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# *Unburden Renters by Making Landlords Pay the Commission?*

# *Evidence from a Natural Experiment*

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# Unburden Renters by Making Landlords Pay the Commission?

# Evidence from a Natural Experiment


Eva M. Berger* and Felix Schmidt*


September 3, 2018


**Abstract**

Recently, prices for rental housing have been increasing in many cities in the western world. Unburdening renters has been the goal of many policy measures, one example is the legal reform "principle who orders pays", introduced in Germany in 2015. The law prescribes that the payment liability of commissions for real estate agents (REA) appointed by landlords can no longer be shifted to renters—as it was common practice earlier. This paper evaluates the effect of the reform on rental prices. Standard economic reasoning predicts the reform to not unburden renters as rents of concerned apartments would increase in a way such that the renters' total burden would stay constant. In contrast, insights from behavioral economics predict no or a weaker increase of rents due to, e.g., inattention or mental accounting. We formalize the hypotheses in a simple model generating a sufficient statistic, i.e., an estimable parameter measuring the degree of imperfect translation between cost components. Based on results from a difference-in-differences estimation, we reject the standard reasoning hypothesis of the hypothesis of a reform-induced rent increase and thus the hypothesis of perfect translation between the cost components. We find the degree of imperfect translation to be substantial (95% lower confidence bound is 73%.) Hence, the reform has effectively unburdened renters. Moreover, the reform is likely to have increased market efficiency as the inefficiently high demand for REA services has been reduced.



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**Keywords:** Policy evaluation, natural experiment, housing rents, real estate agents, liability side equivalence, behavioral bias

**JEL-codes:** R31, D12, H22



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# 1 Introduction

In most developed countries, a significant share of households live in rented (rather than owned) homes. In Germany, this is the case for 48% of all households, overall in the European Union for 30%, and in the U.S. for 36%.[1] Expenditure on housing rents are substantial: in Germany households spend on average 28% of their available income on the monthly housing rent. The share is even larger for low-income households: close to 50% for households with a net income below 700 euros per month.[2] Over the last decade, rental housing became increasingly expensive, especially in large cities. In Germany rents have increased by 30% over the last ten years in cities and 21% in rural areas without agglomerations in West Germany—see Figure 1. Given the high share of expenditure for rental housing and the increasing rental prices, policy makers have been looking for ways to unburden renters. One of the policy measures implemented in Germany was the "principle who orders pays" (*"Bestellerprinzip"*), a legal reform effective since June 2015.

The law prescribes that the commission for real estate agents (REAs) acting on the rental housing market has to be paid by the person who appointed the agent—i.e., in virtually all cases the landlord.[3] Prior to the reform, when landlords appointed an REA to market their apartment,[4] in the vast majority of cases the commission was imposed on the renter and fixed to the maximal legal amount of 2.38 times the monthly rental price (i.e., twice the monthly rent plus the VAT of 19%).[5] This was the case even though the REA provided a service exclusively to the landlord (e.g., publishing the apartment offer, organizing visits, preparing the contract). The REA did not provide any service to the renter; the renter got in contact with the REA only after having found the apartment offer by her own search effort.

The new law was subject to active public debate. A key ex-ante argument by economists against the law was that it would not unburden renters as landlords who appoint an REA (and

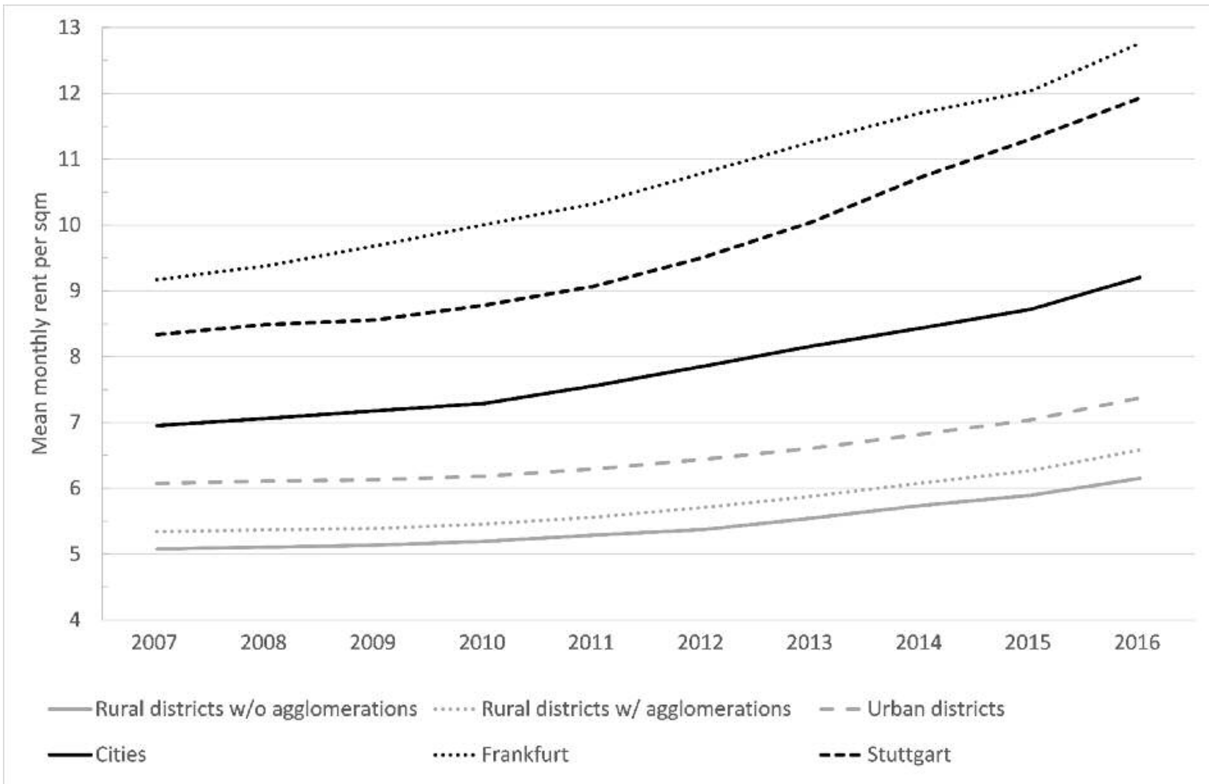
[1]Data from Eurostat and US Census Bureau 2015, see http://www.tradingeconomics.com/european-union/home-ownership-rate.

[2]Rental costs here include net rents plus related costs, such as waste water and garbage collection, but do not include costs for heating and electricity. Data stem from Microzensus Zusatzerhebung 2010; for more details, see Federal Statistical Office (2016).

[3]Strictly speaking, the reform determines that the REA is allowed to rise money from the renter only in the case that the agent interfered with the landlord exclusively for the purpose of the contract with that single renter, cf. *Gesetz zur Dämpfung des Mietanstiegs auf angespannten Wohnungsmärkten und zur Stärkung des Bestellerprinzips bei der Wohnungsvermittlung (Mietrechtsnovellierungsgesetzt—MietNovG)*. An REA interfering with a landlord exclusively for the purpose of the contract with one single renter is a very unrealistic scenario. In fact, since the time of the reform, commissions have no longer been paid by renters (cf., e.g., Michaelis and von Wangenheim, 2016).

[4]In our data, about one third of all apartments were offered with a commission payable by renters, see Section 4 and Appendix B).

[5]Some people feared that after the reform REAs would try to raise money from renters by illegal ways like charging interested persons fees for viewing the apartment, for making the contract, or for administration in general. However, these practices are illegal and REAs risk losing their license and paying monetary fines. Furthermore, renters can claim the money back even three years later. Overall, the illegal fees seem to be rather low and exceptional, see, e.g., Kwasniewski (2016).




Source: Own graph based on data from the Federal Institute for Research on Building, Urban Affairs and Spatial Development (Bundesinstitut für Bau-, Stadt- und Raumforschung—BBSR) . The types of district are officially defined ("Siedlungsstrukturelle Kreistypen").

Figure 1: Development of monthly rental prices per square meter (offer prices) by type of district, West Germany, 2007–2016

now have to pay the commission themselves) would simply increase monthly rents such that the total burden for each party remains constant. This argument—in the following briefly called the standard reasoning argument—is leaned on the liability side equivalence hypothesis from tax incidence analysis stating that the *statutory* incidence of a tax is irrelevant for its *economic* incidence; if, for example, the statutory incidence of a unit tax is shifted from the consumer to the supplier, the price of the consumption good will increase by the amount of the unit tax such that the overall economic liability for each market side is unchanged. Transferred to the REA commission case, the theory proposes that the legal reform "principle who orders pays" does not qualify to unburden renters because increased rents will replace the previous commission burden for renters.[6]

The standard reasoning hypothesis implies that a renter's willingness to pay for the monthly rent is higher if she does not have to pay a commission at the time of signing the rental contract than if she does have to pay a commission. This prediction is based on the assumption that renters have a fixed willingness to pay for the total package 'housing'—including both the (potential) lump-sum commission and the net present value of the periodic rents—and are indifferent about how the total amount payable is distributed between the two. This means that the standard reasoning hypothesis assumes perfect translation between the two cost components.

However, insights from behavioral economics provide reasons to doubt this assumption. For example, people might pay differential attention to the two cost components due to differential salience (for the literature on salience and inattention, see, e.g., Chetty et al. 2009); or, even if people did pay equal (full) attention to both components, they might not be able to calculate the exact amount of rent increase corresponding to the previous commission (phenomenon of cognitive limitations); or, even if people did pay full attention and did have no cognitive limitations, they might *not want* to translate their willingness to pay between the two cost components but rather keep two separate mental accounts (mental accounting phenomenon—see, e.g., Thaler 1999). These arguments might fit either renters' or landlords' reasoning or both and also affect beliefs about reasoning of the other market side. In Section 2 we discuss in more detail the behavioral insights explaining why perfect translation between commission and monthly rents might not be given.

We formalize our hypotheses in a simple model generating a sufficient statistic estimable by a reduced-form regression.[7] The model follows Chetty et al. (2009) and DellaVigna (2009) without limiting the interpretation of the phenomenon of imperfect translation to inattention.

[6]Raising rental prices for new contracts was possible in Germany without restrictions during the main period under consideration. In contrast, rent increases for existing contracts are strongly regulated. Regulations of rents in new contracts, however, have only started with the rent control law of 2015 (*"Mietpreisbremse"*), which, however, has been effective in the regions under consideration only since November 2015 (and this change is controlled for in the empirical analysis, see below).

[7]Chetty (2009) and DellaVigna (2018) promote the use of sufficient statistics as a combination of the advantages of structural models and pure reduced-form approaches.

In this paper we test the null hypothesis of rents increasing by the rate predicted by the standard reasoning argument against the alternative hypothesis of rents increasing not or to a smaller extent, leaning on arguments from behavioral economics. The exact price increase predicted by the standard reasoning hypothesis depends on the (expected) rent duration. In the case of a mean expected rent duration of ten years, i.e., 120 months,[8] and an interest rate of zero (most conservatively), we should observe an increase in rental prices by 1.98% (= 2.38/120),[9] [10] We thus test the null hypothesis of perfect translation against the behavioral hypothesis of imperfect translation between the two cost components. Note that we do not explain the exact behavioral mechanism underlying imperfect translation; this is due to the available data and to the fact that the behavioral phenomena (discussed in Section 2) all point to the same direction. The aim of this paper is rather to pin down whether the phenomenon of imperfect translation exists (even in the high stakes decision of rental housing) and to measure about its size. This finally allows us to answer the question of whether a policy reform as the one under consideration is able to unburden renters or not.

For our empirical analyses, we use data from the online real estate marketplace Immobilienscout24 and apply a difference-in-differences approach. Based on a panel dataset of apartments advertised both at least once before the reform and at least once after the reform, we compare the rental price development of apartments that were offered with a commission prior to the reform (treatment group, affected by the reform) to the rental price development of apartments that were offered without a commission prior to the reform (control group, not affected by the reform). The panel structure of the data allowing to include apartment fixed effects comes with the major advantage of allowing to control for all time-invariant unobserved apartment characteristics which potentially affect both rental prices and the marketing channel (i.e., REA versus no REA).

Our results suggest that the null hypothesis has to be rejected at all conventional levels of statistical significance: We find no increase in rents due to the legal reform, not even in the long-run. The mean expected rent duration would need to be at least 50 years (i.e., implausibly high) in order that the null hypothesis could not be rejected at the 5% level. Relating back to tax incidence analysis, our finding is consistent with recent evidence based on laboratory experiments (Cox et al., 2017) and natural experiments (Saez et al., 2012) suggesting that tax incidence is not independent of the assignment of the liability. Our finding of an imperfect translation between cost components implies a market inefficiency prior to the reform because the subject benefiting from the service (i.e., the landlord) did not (fully) bear the cost of the

[8] In Section 6 we discuss the reasons for choosing $D$ to be equal to 120 months and provide sensitivity tests.

[9] Remember that the REA commission prior to the reform amounted virtually always to 2.38 times the monthly rental price.

[10] In the presence of consumer myopia (i.e., discounting of future payments by a rate larger than the interest rate—which we assume to be zero), the standard reasoning prediction would postulate an even higher price increase than 1.98%. The same is true if renters have credit constraint reducing their ability to afford the lump sum REA commission.

service; this likely led to an inefficiently high demand for REA services. This is consistent with the observed fact that a decreasing number of apartments is offered by REAs after the reform. Given these considerations, the legal reform potentially improved the efficiency of the market and thus overall welfare.

To the best of our knowledge, there is no previous study investigating the effect of shifting the payment liability of the REA commission between renter and landlord nor is there evidence of the effect of shifting the payment liability between seller and buyer in the housing selling market. In the rental housing market of a number of countries, such as Austria, Finland, Denmark, France, Luxemburg, Italy, and Sweden, the commission for REAs appointed by landlords is (partly) paid by renters. This is the case even though in most developed countries, the REA profession is regulated to some extent (CEPI, 2013). Hence, investigating the effect of changes in regulations such as the legal reform studied in the present paper is relevant. It shows that a simple shifting of the payment liability between market sides can effectively unburden one side—unlike predicted by standard economic reasoning.

Due to the large amount of money spent on housing rents (see beginning of this section), revealing behavioral biases in decisions of actors on the rental housing market is economically relevant. We are unaware of any previous study examining behavioral biases on the rental housing market;[11] in contrast, there is some evidence of behavioral biases of actors on the housing *selling* market: Genesove and Mayer (2001) found that loss aversion determines seller behavior as owners subject to nominal losses set higher prices and incur a longer time on the market than other sellers. Bucchianeri and Minson (2013) report a positive relationship between listing prices and sale prices consistent with the literature on anchoring effects. Brunnermeier and Julliard (2008) found that money illusion explains a substantial part of the sharp run-ups and downturns in the housing market.[12] Given that understanding behavioral reactions is key for evaluating the effectiveness of policy measures, the present study provides a significant contribution for understanding the effectiveness of policies in the domain of housing.

The paper is organized as follows: In Section 2 we outlay some key reasons based on insights from behavioral economics why the legal reform of the "principle who orders pays" potentially does unburden renters—against the prediction of standard economic reasoning. In Section 3 we provide a formal framework to present our hypotheses and structure thoughts. Section 4 describes the data and presents some descriptive analyses. Section 5 gives details

[11]Rather than behavioral phenomena, the literature in the field of rental housing focuses on topics such as the general development of rental prices over time or across regions (e.g., Himmelberg et al., 2005; Ambrose et al., 2015; Thomschke, 2015), the natural vacancy rate (e.g., Rosen and Smith, 1983), investment incentives in real estate depending on ownership (e.g., Gebhardt, 2013), or the effect of government interventions like rent control (e.g., Arnott and Igarashia, 2000; Kholodilin et al., 2016; Sims, 2007; Thomschke, 2016) and housing vouchers (e.g., Eriksen and Ross, 2015).

[12]Glaeser and Nathanson (2015) review rational and nonrational models that have been developed to explain housing bubbles. Salzman and Zwinkels (2013) give an overview how behavioral bias affects real estate finance and investment decisions.

about the empirical strategy: a difference-in-differences strategy with apartment-fixed-effects. Section 6 presents and discusses the results and robustness tests. Section 7 concludes.

# 2 Reasons Why the Reform Could Unburden Renters—the Behavioral Hypothesis

The standard reasoning argument predicts the law "principle who orders pays" to increase rents in a way that keeps the total burden for each market side constant; this would imply the law not to unburden renters. The standard reasoning argument relies on the assumption of perfect translation between the (lump-sum) commission and the (monthly) rent. In this section we discuss three key arguments challenging this assumption and thus being arguments why the reform could reach its goal of effectively unburdening renters. As noted before, we are not able to empirically distinguish between different behavioral mechanisms. The arguments presented in this section were merely the motivation for investigating on the standard reasoning hypothesis. The purpose of this paper is to test the validity of the standard reasoning hypothesis in order to be able to answer the question of whether the reform unburdened renters effectively—as intended by politicians but contradicting the standard reasoning hypothesis—or not.

The first argument why rents could increase less than predicted by the standard reasoning hypothesis is based on the different salience of cost components and the associated different levels of attention consumers pay to these cost components. People looking for an apartment are likely to focus more on the rental price than on side costs such as a commission payable at the time of contract conclusion. This means that the price for the commission is less salient than the rental price, and future renters thus pay more attention to the rental price than to a commission they are required to pay for some apartments but not for others. We do not argue that the amount of the commission is not transparent, it is fully transparent, but people might just pay less attention to it with respect to their apartment decision. When people look for an apartment, they often have in mind some maximum rental price they are willing to pay. But they rarely have in mind two different maximal rental prices, one that applies for apartments offered without a commission, and another (lower) maximum amount that applies for apartments offered with a commission payable by the renter.[13]

The phenomenon of different salience affecting consumer behavior has been found to affect online purchases, where consumers pay more attention to the item's direct price than to the shipping cost—even though the latter is directly related to the purchase (Hossain and Morgan, 2006; Brown et al., 2010). Dertwinkel-Kalt et al. (2017) show that salience plays a role for purchases of two vertically differentiated products. Other work has shown that sales taxes can be less salient than an item's net price and that this importantly affects behavioral responses to

[13] Remember, in the case that a commission had to be paid, this usually amounted to 2.38% of the monthly rent.

taxation, i.e., tax incidence (Chetty et al., 2009). Relatedly, the salience of the toll collection system has been reported to influence the elasticity of demand for tolled roads and thus equilibrium tax rates (Finkelstein, 2009). On the used car market buyers have been found to pay more attention to the first digit of the mileage of used vehicles than would be predicted by rational theory and this was also explained by the higher salience of the first digit compared to later digits (Lacetera et al., 2012; Busse et al., 2013).

In the apartment rental decision analyzed in the present paper, a lower salience of the commission compared to the rent would imply the following: if renters were completely inattentive to the commission, their willingness to pay for the rent of apartments with commission would be equal to the willingness to pay for the rent of apartments without commission. Hence, their willingness to pay for the rent would not increase due to the reform. In the less extreme case, if renters paid some but not full attention to the commission cost, the willingness to pay for apartments that previously had a commission payable by renters would increase due to the reform, but the increase would be weaker than predicted by standard economic reasoning. The exact increase would depend on the degree of attention paid to the commission relative to the attention paid to the rent. Apart from renters, landlords might also be inattentive to the (less salient) commission and therefore not decrease rents in cases where the renter pays the commission for them; or, said differently, not increase rents (enough) as soon as renters stop paying the commission due to the reform. Even if landlords were perfectly attentive but had correct beliefs about the renters' inattention towards a commission, it would be rational for landlords to make renters pay the commission (prior to the reform) rather than paying it themselves because the realistically possible increase in rent in the case of the landlord paying the commission would be worth less than the commission in the case of inattentive renters. This is the situation prevalent prior to the reform: landlords who appointed an agent (close to) never paid the commission themselves but always made renters pay the commission. If the standard reasoning hypothesis perfectly applied, however, they should be indifferent. The different salience and inattention could thus explain why the reform did not make rents increase as predicted by the standard reasoning hypothesis und why the reform thus could effectively unburden renters.

A second argument why one might hypothesize the standard reasoning hypothesis not to apply is cognitive limitations. Even if renters and landlords were fully attentive to the REA commission (i.e., even if the first argument did not apply), they might simply not be cognitively able to translate a lump-sum cost (the commission) into the equivalent periodic (monthly) amount (the rent). Recent research shows that there seems to be a relationship between the application of cognitive resources and the expression of behavioral biases (Benjamin et al., 2013; Brown et al., 2017). Renters might be unable to calculate the amount that their willingness to pay for the rent should increase when not being required to pay a commission and this inability might make them simply neglect this amount. The argument analogously applies to landlords who might be unable to calculate the amount of money their asking rent should increase at the moment when the liability to pay the commission is shifted from the renter to the landlord. If the cognitive inability of translating a lump-sum amount into a monthly amount (and potentially even taking into account the interest rate and different scenarios of expected rent durations) led to a systematic neglect, this would explain why rents did not increase as a result of the reform.

A third argument why the reform would not affect rents and thus could effectively unburden renters is based on the behavioral concept of mental accounting, established by Richard Thaler (Thaler, 1980, 1985, 1999). Even if renters and landlords were perfectly attentive (unlike the first argument) and cognitively able to do the math (unlike the second argument), they might simply not *want* to translate the commission into the equivalent rent increase because they have different mental accounts for these different cost components. Empirical evidence suggests that people keep spending under control by forming multiple mental accounts (e.g., for food, housing, etc.) between which money is not (or not fully) transferable. "Mental accounting violates the economics notion of fungibility" as "money in one mental account is not a perfect substitute for money in another account" (Thaler, 1999, p. 185). Mental accounting thus affects consumption choices. Almenberg and Karapetyan (2010) show that mental accounting affects borrowing behavior in the housing selling market.

Applied to the rental housing decision, we propose that people searching for an apartment to rent assign the expenses for the commission to a different mental account (say the account "moving expenses") than the expenses for the monthly rent (say the account "regular rental expenses"). This is particularly likely as the two accounts have different time horizons: the first account encompasses one-time expenses for the time of moving, while the second account encompasses periodic (monthly) expenses. These different time horizons complicate translation between the accounts (Prelec and Loewenstein, 1998). If people do not transfer money between the two mental accounts, renters' willingness to pay for the rent is the same in the case that the apartment is offered with a commission payable by the renter as in the case that the apartment is offered without a commission. Thus, their willingness to pay for the rent of an apartment that previously had a commission does not increase when no longer required to pay a commission (as determined by the reform). If money is transferred partly but not fully between the two mental accounts, their willingness to pay increases at the time of the reform but less than predicted by the standard reasoning argument. The exact level of rent increase of apartments that previously had a commission depends on the degree of transferability between the accounts.

We believe that the presented three arguments are the main reasons why the reform is likely to make rents increase less than predicted by standard reasoning. Certainly, we cannot rule out other arguments like landlords and/or renters systematically overestimating rent durations. But, as will be shown in the results section, the rent duration would be needed to be estimated to at least 50 years in order that the standard reasoning hypothesis could not be rejected. This appears rather unrealistic.

Another—non-behavioral but rational—reason for renters to account for commission costs differentially than for the rent could be employers paying for commission costs. This, however, is certainly relevant only for cases of job-related moving and among these only for cases where the employer pays for moving costs. Evaluating data from the representative household survey SOEP, we find that only 15% of household moves in 2012/2013 were due to job-related reasons.[14] Among these job-related moves, certainly not all employers pay for moving costs; and among those cases where employers pay for moving costs, many accord their employees flat rates that are independent of the actual expenses. Employees therefore still have an incentive to avoid REA commissions. Hence, job-related moving is little reason for renters not to take into account the cost for commissions.

# 3 Formal Framework

In this section we provide a simple framework to structure thoughts and formalize our hypotheses. We do not provide a fully specified model of the market. For analyzing the effect of the reform under consideration on rents we follow the avenue proposed by Chetty (2009) and DellaVigna (2018), focussing on a sufficient statistic with a parameter that can be estimated by reduced-form regressions.

In the following, we first provide the framework under the null hypothesis, i.e., under standard economic reasoning. We then release the key assumption—we call it the perfect translation assumption—of this standard economic framework and introduce the parameter $\theta$ measuring the degree of imperfect translation.

## 3.1 Framework Under Standard Economic Reasoning

Consider a competitive market where the overall price for living in a certain apartment to rent is equal to the renter's willingness to pay.[15] In the case of no commission payable by the renter, this overall price purely consists of the (net present value of the) rents. In the case of a commission payable by the renter, the overall price consists of both the (net present value of the) rents and the commission. Expressed in monthly terms, this means that $V = P + \frac{C}{D}$, where $V$ is the renter's monthly willingness to pay, $P$ is the monthly rent, $C$ is the lump-sum commission

[14] German Socio-Economic Panel study, data for years 1984-2013, version 30, SOEP, 2014, doi:10.5684/soep.v30, see also Wagner et al. (2007); own calculations.

[15] Certainly, in a search market with heterogeneous goods, the overall price might be below the willingness to pay. This, however, is not critical for our analysis as long as the reform under consideration does not change central parameters of the market such as search costs occurring at the renter or the distribution of apartment characteristics. The distance between the final price and the willingness to pay is assumed to be unaffected by the reform.

payable by the renter (which is zero in the case of no commission payable), and $D$ is the rent duration in months.[16] Rearranging yields

$$P = V - \frac{C}{D}. \tag{1}$$

This implies that the rent is higher in the case *without* commission payable by the renter (i.e., $C = 0$, implying $P = V$) than in the case *with* commission payable by the renter (i.e., $P = V - \frac{C}{D}$). Consequently, abolishing commissions payable by renters (as the reform does) should increase rents of apartments that, prior to the reform, did have a commission payable by renters, by exactly the amount $\frac{C}{D}$.

Considering landlords' decision prior to the reform about whether to appoint an REA (with the commission payable by the renter) or not, we note the following: In the case of appointing an REA, the landlord's net earnings (in monthly terms), $\pi$, are equal to the monthly rent earned, i.e., $\pi_{REA} = P_{REA}$. Inserting $P$ from (1) yields

$$\pi_{REA} = V - \frac{C}{D}.$$

In the case of appointing no REA and marketing the apartment on his own, the landlord's net earnings are equal to the monthly rent minus the one-time expenditure for marketing the apartment on his own, $E$, divided by the rent duration $D$, i.e., $\pi_{noREA} = P_{noREA} - \frac{E}{D}$. Inserting $P$ from (1) with $C = 0$ yields

$$\pi_{noREA} = V - \frac{E}{D}.$$

This implies that, prior to the reform, a landlord will decide to appoint an REA if $E > C$.

After the reform, landlords are still free to appoint an REA, but, in the case they do, they have to pay $C$ themselves to the REA. This means that, for deciding whether to appoint an REA or not, landlords directly compare the commission $C$, which they have to pay to the REA in the case of appointing one, with the cost $E$ occurring in the case they do not appoint an REA but market the apartment on their own. Hence, the decision criterion for landlords to appoint or not an REA is equal before and after the reform. The only difference is that, before the reform, landlords pay $C$ indirectly through a lower monthly rent earned, while after the reform, landlords pay the commission directly.[17]

In the simple framework presented we make the following assumptions: First, we assume that renters do not derive any utility or disutility from the apartment being marketed by an REA

[16]For simplicity we set the interest to zero. First, this is very close to reality at that time. Second, if we release this assumption and assume a positive interest rate, the rent increase predicted by the standard reasoning hypothesis is even higher. Thus, the zero-interest assumption is most conservative for our analysis.

[17]Note, the reform does not change the service brought by the REA nor the value the landlord assigns to this service. Furthermore, as discussed in section 1, the REA does not provide any service to the renter; the renter gets in touch with the REA only after having found the apartment offer on his own search initiative.

versus being marketed by the landlord on his own.[18] Second, we assume that the reform does not affect other parameters of the model, in particular $V$ or $D$.[19] Third, the crucial assumption in this framework under standard economic reasoning is the assumption about perfect translation between rent and commission. It says that renters' demand depends solely on the total cost consisting of the *sum* of rent and commission (divided by the number of months) and is independent of the distribution between the two cost components. If, however, renters perceive these two cost components differently and thus make them differentially influence demand, the assumption is violated. The assumption of perfect translation is what we test in the empirical part of this paper. In the following, we therefore provide a framework allowing for imperfect translation between the two cost components.

## 3.2 Framework with Imperfect Translation Between Cost Components

The reasons for imperfect translation between the cost components are manifold and could occur for consumers (renters) or suppliers (landlords). As discussed above, reasons for imperfect translation could be, for example, (partial) inattention to the commission (due to lower salience), cognitive limitations, or mental accounting. To account for imperfect translation between cost components, we adapt our framework by introducing the parameter $\theta$, interpreted as the degree of imperfect translation. The model follows Chetty et al. (2009), DellaVigna (2009), Finkelstein (2009), and Lacetera et al. (2012), who, however, interpret imperfect translation to be due to consumers' inattention to some opaque component of a variable compared to full attention to the visible component of that variable. In our context, $\theta$ does not need to be interpreted narrowly as the degree of inattention; it could alternatively be interpreted, for instance, as the degree of intransferability between mental accounts or the degree of cognitive inability to translate between cost components. We refrain from restricting our interpretation to a specific underlying psychological mechanism and thus call $\theta$ simply the degree of imperfect translation.

In this framework we make the same assumptions as in the previous framework under standard economic reasoning, but we release the perfect translation assumption. A renter's monthly willingness to pay is now $V = P + (1 - \theta)\frac{C}{D}$. With $\theta \in (0, 1)$, the demand depends now to a

[18]This is likely to be very close to reality. From anecdotic evidence it seems that if anything, renters dislike interacting with REAs (compared to interacting with landlords). This is among other things because REAs tend to organize apartment visits with several interested parties instead of with one individual person; also, many renters prefer to meet the landlord early in the process in order to get to know the contractual partner and thus the person they have to get along with during the entire tenancy duration. If we did not assume that renters are indifferent but that renters dislike apartments being marketed by REAs, the reform-induced rent increase would be predicted to be even stronger than otherwise. Thus, our assumption of renters' indifference is rather conservative.

[19]The assumption that preferences for apartment characteristics are unaffected by the reform is rather uncritical. The assumption of $D$ being unaffected refers to the rent duration prior to the reform and is thus uncritical as well. The extent of the predicted rent increase depends on the rent duration prior to the reform only rather than the rent duration after the reform. It is the rent duration prior to the reform that determines the degree of rent the landlord forgoes in order to use the REA services. It is exactly this forgone rent that is expected to be caught up by a rent increase after the reform.

smaller extent on the monthly representation of the commission ($\frac{C}{D}$) than on the rent ($P$). For $\theta = 0$ the model reduces to the standard economic model presented above. In contrast, $\theta = 1$ would imply no translation between cost components at all. Since we now have

$$P = V - (1 - \theta)\frac{C}{D},$$

the reform is expected to raise $P$ by $(1 - \theta)\frac{C}{D}$ (for apartments with a commission payable by the renter prior to the reform). Hence, this model predicts the reform-induced rent increase to be smaller than the standard reasoning model does.

In addition, in this framework, the reform changes landlords' decision criterion for appointing an REA. Before the reform, appointing an REA made landlords earn

$$\pi_{REA} = P_{REA} = V - (1 - \theta)\frac{C}{D},$$

while appointing no REA made them earn

$$\pi_{noREA} = P_{noREA} - \frac{E}{D} = V - \frac{E}{D}.$$

This implies that landlords used to appoint an REA if $E > (1 - \theta)C$. This means that landlords did not account for the full commission cost when deciding for or against an REA prior to the reform.

After the reform, as landlords now have to pay the commission directly, the decision criterion for appointing an REA is simply $E > C$. Hence, landlords with $(1 - \theta)C < E < C$ should have appointed an REA prior to the reform but will not do so after the reform. These cases constitute an inefficiently high demand for REA services prior to the reform as for these cases the cost of the REA service is larger than its benefit. The model predicts the reform to reduce this inefficiently high demand for REA services. Note, however, that the predicted decline in the number of appointed REAs is irrelevant for the prediction about the rent increase. The rent increase depends solely on the commission status prior to the reform. No matter what a landlord spends the additional rent on after the reform (REA commission or marketing the apartment on his own), the model predicts the reform to raise $P$ by exactly $(1 - \theta)\frac{C}{D}$.

In the following sections we use a panel data set of offer rents to estimate the reform-induced rental price change and thereby estimate the parameter $\theta$. Finding the rental price increase to be equal to 1.98% and thus $\theta$ to be equal to zero would be consistent with perfect translation and thus with the standard reasoning model outlined in Section 3.1.

# 4 Data and Descriptive Evidence

## 4.1 Data Description

The dataset used for our empirical analysis was provided by the firm Immobilienscout24, which offers by far the largest online real estate marketplace for residential properties in Germany.[20] Due to constraints of Immobilienscout24, the firm was willing to provide data only for two cities, which we chose to be Frankfurt and Stuttgart. We chose to focus on large cities instead of rural areas because more than 35% of the German population is currently living in cities (Eurostat, 2016). Also, renting instead of owning an apartment or house is much more common in cities than in rural areas.

We specifically chose the cities of Frankfurt and Stuttgart because of two reasons: First, rent dynamics have been particularly strong in these cities (see Figure 1) suggesting that demand is high. Moreover, Kholodilin (2012) provides evidence that rents in these cities tend to be underpriced compared to many other large cities in Germany and Europe. Thus, the supply side on the rental housing market is likely to be strong enough for landlords to be able to easily raise rents. If we can reject the standard reasoning hypotheses for these two cities, it is unlikely that we cannot do so in other regions in Germany.

Second, at the date when the law "principle who orders pays" became effective, a rent control law was decided on at the same time. The rent control law had the aim of decelerating rent growth. It allowed federal states to define communities or residential districts in which rents should be controlled. In these controlled areas rental prices of new rental contracts are then prohibited to surmount 10% of the local average calculated by local authorities. Exceptions are generally conceded to new buildings (built after 2014). It is important to note that the rent control law became effective only when local governments (governments of the federal states) decided to implement it. In the federal state of Hesse (where Frankfurt is located in), the rent control law became effective only on November 27, 2015, and in the federal state of Baden-Württemberg (where Stuttgart is located in), the rent control law became effective only on November 1, 2015.[21] The time distance between the two laws, the "principle who orders pays" and the rent control law, allows us to disentangle their effects—which is not possible in other large cities in Germany.[22] Having a time distance between the two laws is crucial, in particular

[20]The market share amounts to 63% with more than 13 million users looking for a new home via immobilienscout24 every month, see: https://www.immobilienscout24.de/werbung/scout24media/plattformen/immobilien scout24.html.

[21]In order to control for the effect of the rent control law in our sample, we construct a dummy variable indicating whether the rent control law affects an offer. According to the law, this dummy variable depends on time of the offer, building year, and exact location. In Frankfurt, not all districts are subject to the law: the districts that were decided (by the local government) not to be subject to the rent control law are Berkersheim, Eckenheim, Harheim, and Unterliederbach.

[22]In other large cities in Germany, both reforms became effective simultaneously or shortly one after another: e.g., in Berlin the rent control law became effective on June 1, in Hamburg on July 1, in Cologne (North Rhine-Westphalia) also on July 1, in Munich (Bavaria) on August 1.

since the rent control law was supposed to have a negative effect on rents;[23] for regions where both laws became effective at the same time we would be unable to identify the pure effect of the legal reform "principle who orders pays" on rents just because of the distorting effect of the rent control law.[24]

Our full dataset contains N=219,247 rental offers published on the Immobilienscout24 website between January 2012 and June 2016.[25] The apartment price information included in the dataset is the monthly rental price excluding service charges. Monthly rental prices on Immobilienscout24 are asking prices. We do not have explicit information about transaction prices. However, assuming that asking prices are equal (or very close) to transaction prices is plausible as negotiations about rents are very uncommon in Germany, especially in large cities. Comparing the asking prices in our dataset to official statistics about rents from the Federal Institute for Research on Building, Urban Affairs and Spatial Development (Bundesinstitut für Bau-, Stadt- und Raumforschung—BBSR), we find that they are very close.[26]

Table 1 reports summary statistics of all relevant variables in our dataset. Since most variables stem from non-mandatory fields, we face some item nonresponse, which can be seen from the last column of Table 1. The average rental price amounts to 997 euros. 95% of all observations are in the price range of 350 to 2,380 euros. Figure 2 (a) displays the distribution of monthly rental prices.[27] The histogram is bell-shaped and clearly skewed to the right. The average price per square meter (sqm) amounts to 11.9 euros. 95% of all observations are in the price

[23]Empirical studies, however, found that the effect of the rent control law was moderate (Kholodilin et al., 2016; Mense et al., 2017; Thomschke, 2016).

[24]Anticipation of the rent control law should not be a problem for our analyses for the following reasons: First, there is no reason for landlords to decrease rents even before a rent control becomes effective. If anything, they have reason to increase rents because they will not be able to do so later. However, increasing rents would be supportive of the standard economic hypothesis; being able to reject this hypothesis will be an even stronger finding. Second, there is no reason for anticipation effects of the rent control law to affect apartments with commission differently than apartments without commission; this is, however, what our difference-in-differences analysis is based on.

[25]This is the sample size we get after dropping some observations with extreme values that are most likely due to typos: We dropped observations with an apartment size of less than 10 square meters (sqm) (128 cases; this is very conservative as most of these observations have a 0 or 1 in the variable sqm; the 1-percentile of sqm is 20) or with an apartment size > 400 sqm (76 cases; again very conservative as the 99-percentile is at 230). We further dropped observations with a price per sqm of less than 4 euros (another 2183 cases; the 1-percentile is at 4.11 euros) or more than 30 euros (another 551 cases; the 99-percentile is at 24 euros). We also dropped observations where the number of rooms was indicated to be smaller than 1 or larger than 15 (another 45 cases). Finally, we dropped observations where the average room size is less than 10 sqm (another 73 cases; these most likely contain typos in the number of rooms and/or sqm). Cleaning the data as described for some obvious typos or spammers is reasonable as most data fields are freely filled by the offeror and thus prone to typos and also because some spammers offer apartments with unrealistic features. Also, we dropped observations with missing values in our main variables rental price, post reform, commission, number of rooms, and square meters (another 1,665 cases) as these variables are used to construct the panel dataset (see Section 5.2) and to apply the diff-in-diffs strategy. The number of observations dropped due to cleaning and missing variables is very low compared to the overall sample (in total 4,721 out of originally 223,968 observations, i.e., around 2%). Even when we do not clean the data as described here, our main results are very similar—as we have found in a robustness check.

[26]In our dataset the mean rent per sqm is EUR 11.9; in the official data the mean rent per sqm in Frankfurt in 2012 is EUR 10.8 and increases to EUR 12.8 in 2016. In Stuttgart it is EUR 9.5 in 2012 and increases to 11.9 in 2016.

[27]For the Figure, we cut at a price level of 4000.

Table 1: Summary statistics

| Variable | Mean | SD | Min | Max | N |
|---|---|---|---|---|---|
| Rental price (in euros) | 996.251 | 709.76 | 50 | 9000 | 219,247 |
| Price per sqm (in euros) | 11.903 | 3.586 | 4 | 30 | 219,247 |
| Post reform | 0.187 | 0.39 | 0 | 1 | 219,247 |
| Commission[a] | 0.547 | 0.498 | 0 | 1 | 219,247 |
| Frankfurt (vs Stuttgart) | 0.687 | 0.464 | 0 | 1 | 219,247 |
| Number of rooms | 2.671 | 1.099 | 1 | 13 | 219,247 |
| Size in square meters | 81.737 | 41.222 | 10 | 400 | 219,247 |
| Floor level | 2.362 | 1.947 | -1 | 24 | 176,418 |
| Total number of floors in house | 4.268 | 2.308 | 0 | 50 | 146,864 |
| Balcony | 0.683 | 0.465 | 0 | 1 | 199,132 |
| Garden | 0.173 | 0.378 | 0 | 1 | 171,310 |
| Number of bathrooms | 1.194 | 0.414 | 1 | 5 | 144,197 |
| Guest bathroom | 0.284 | 0.451 | 0 | 1 | 198,827 |
| Elevator | 0.368 | 0.482 | 0 | 1 | 189,346 |
| Cellar | 0.696 | 0.46 | 0 | 1 | 207,866 |
| Built-in kitchen | 0.711 | 0.453 | 0 | 1 | 194,016 |
| Parking space | 0.002 | 0.043 | 0 | 1 | 219,247 |
| Inner-city | 0.53 | 0.499 | 0 | 1 | 219,247 |
| Rent control | 0.092 | 0.289 | 0 | 1 | 219,247 |
| Private offer | 0.187 | 0.39 | 0 | 1 | 214,580 |
| Offer by REA | 0.567 | 0.495 | 0 | 1 | 214,580 |
| Offer by other commercial entity | 0.238 | 0.426 | 0 | 1 | 219,247 |

[a] Within only the sample prior to the reform the share of offers without commission is 0.67. Data from Immobilienscout24 2012–2016, authors' calculations.

range of 6.67 to 18.49 euros per sqm. The distribution of monthly rental prices per sqm can be seen in Figure 2 (b). The distribution is bell-shaped and close to symmetric. 81.3% (18.7%) of the apartment offers were published before (after) the reform became effective at June 1, 2015. For more than half of all offers (54.7%) a commission had to be paid by renters; the percentage was 58.8% before the reform and 1.0% after the reform.[28] 18.6% of the apartments were offered privately, 56.8% by REAs, and 23.8% by other commercial entities (in particular, house constructing enterprises). 9.1% of all offers were potentially affected by the rent control law. The average apartment offered has 81.7 square meters and 2.67 rooms. 95% are in the range of 1 to 4.5 rooms and in the range of 30 to 158 sqm. The histograms in Figure 2 (c) and (d) display the distributions of the number of rooms and number of sqm. The distributions are bell-shaped and the means seem reasonable: The Federal Statistical Office reports from its survey of 2011 on all apartments a mean number of rooms of 3.5 in Frankfurt and 3.8 in Stuttgart; the figures of the Federal Statistical Office include the kitchen, while the numbers in our dataset exclude the kitchen. The mean apartment size in sqm has been found by the Federal Statistical Office to be 72.9 in Frankfurt and 78.2 in Stuttgart (Federal Statistical Office, 2015).

## 4.2 Descriptive Analysis Over Time

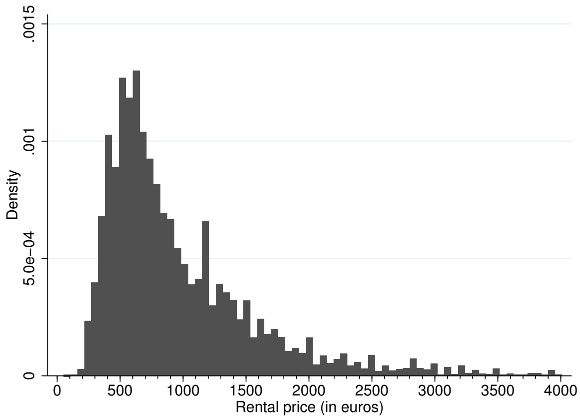
Since the law “principle who orders pays” became effective nationwide at the same date (June 1, 2015), we illustrate the development on the rental market graphically. Figure 3 displays the development of rental prices over time.[29] The day the law “principle who orders pays” became effective is indicated by the red vertical line; the two gray lines indicate the implementation of the rent control law in Frankfurt (November 2015) and Stuttgart (December 2015).[30] Prices were continuously increasing prior to the reform and there is a peak at the exact time of the reform; however, the high level is not durable. Therefore, no clear price increase can be graphically inferred from the reform. At the end of the year 2015 there is a sharp decline in rental prices; this coincides with the implementation of the rent control law. However, after a strong decline, prices start to recover.

The fluctuation in rental prices could be due to fluctuating characteristics of the mean apartment over time. Hence, in order to reduce variation and increase precision, we further plot the residual of a regression of the natural logarithm of the rental price on a number of apartment

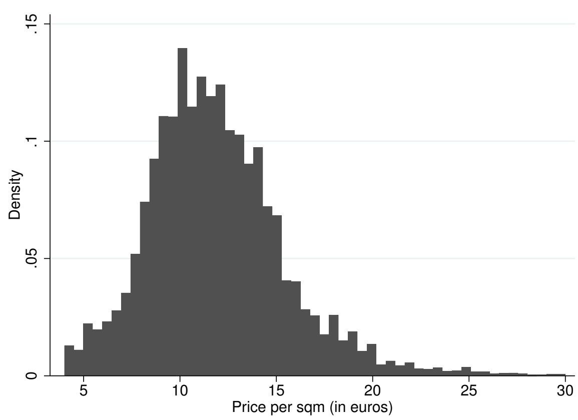
[28] As the firm Immobilienscout24 (that we got the data from) told us, the 1% is either due to errors (e.g., people re-publishing an offer they already had published once before the reform and they now missed to delete the commission requirement) or due to attempts to ignore the reform.

[29] This and the following graphs shown in this section are also plotted separately by city, see Figures A1 to A4 in Appendix A.

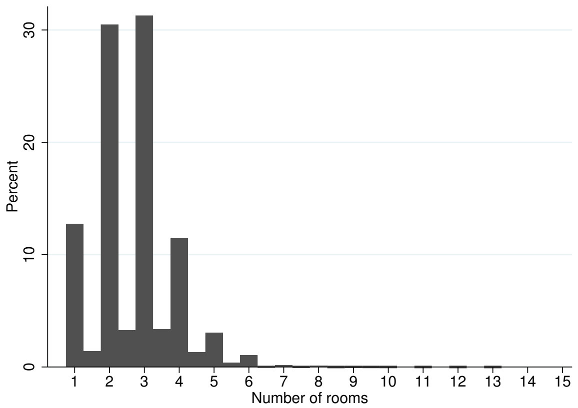
[30] Our data contains only the month and not the exact day each offer was published online. Therefore we round the rent control law start time to be December instead of November 27, assuming that this produces a measurement error of negligible size.



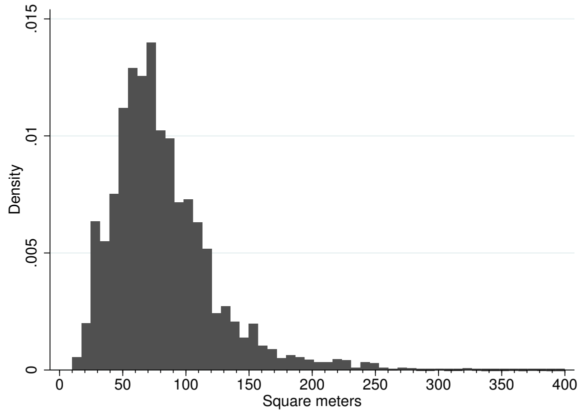

(a) Monthly rental price (in euros)




(b) Price per square meter (in euros)




(c) Number of rooms



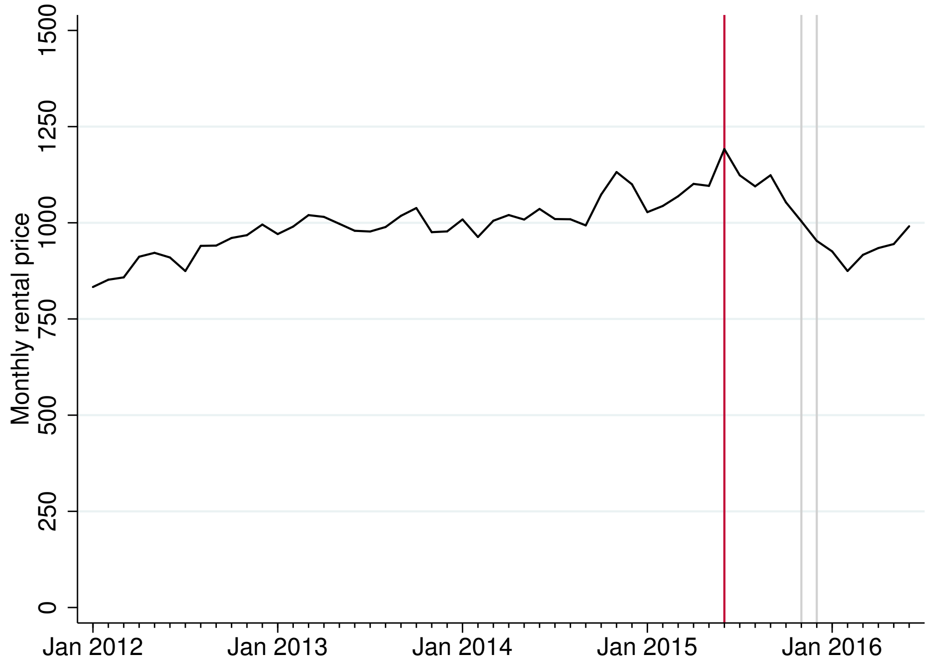

(d) Apartment size (in sqm)

Note: N = 219,247, data from Immobilienscout24, authors' calculations.

Figure 2: Distributions of apartment characteristics

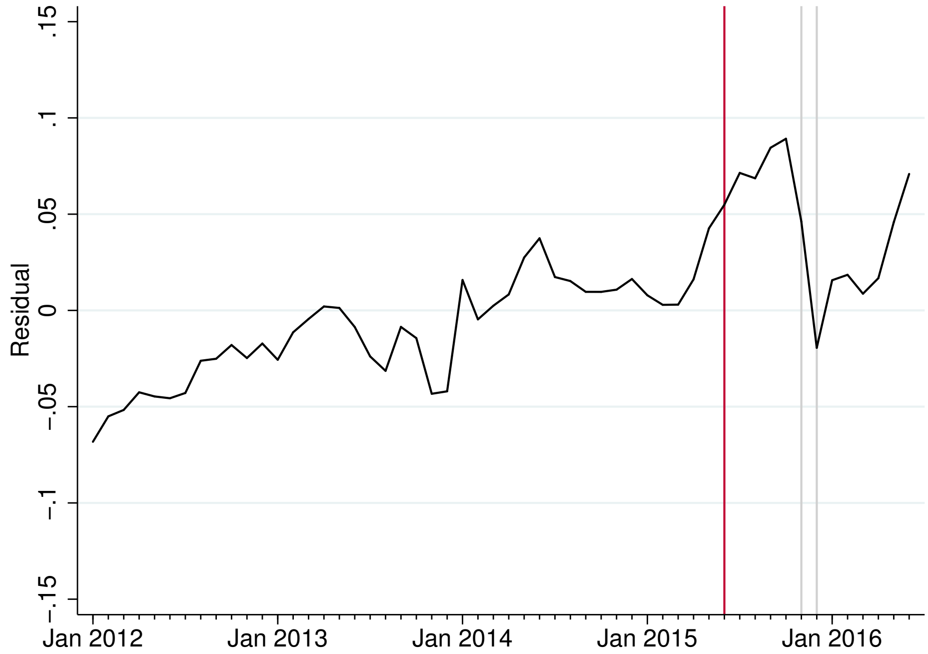
characteristics[31] (Fig. 4). Again, it is hard to see from the graph whether the price increase at June 2015 reflects the overall time trend or is (additionally) due to the law "principle who orders pays". There is no clear discontinuity at the day the law became effective.

[31]The regressors include: city (dummy variable for Frankfurt vs Stuttgart), ln(square meters), number of rooms (dummy variables), floor level (six dummy variables for the floor levels between -1 and 4, one dummy variable for floor levels between 5 and 9, one dummy variable for floor levels of 10 or larger, and one dummy variable for missing information on floor level), total number of floors in house (six dummy variables for the number of floors between 0 (only ground floor) and 7, one dummy variable for 8 or more floors, and one dummy variable for missing information on the number of floors), balcony (dummy variables for yes and no and missing information), garden (dummy variables for yes and no and missing information), interaction between balcony and garden, number of bathrooms (dummy variables for 1, 2, 3, and 4 or more, and a dummy variable for missing information), guest bathroom (dummy variables for yes and no and missing information), elevator (dummy variables for yes and no and missing information), cellar (dummy variables for yes and no and missing information), built-in kitchen (dummy variables for yes and no and missing information), parking space (dummy variables for yes and no and missing information).




Note: The red vertical line indicates the point in time when the law “principle who orders pays” became effective (June 2015), the two gray lines indicate the points in time the rent control law became effective in Frankfurt (December 2015) and Stuttgart (November 2015). N = 219,247, data from Immobilienscout24, authors’ calculations.

Figure 3: Mean rental prices over time




Note: Residuals from a regression of ln(rental price) on city (dummy for Frankfurt vs Stuttgart), ln(square meters), number of rooms (dummy variables), floor level (six dummy variables for the floor levels between -1 and 4, one single dummy variable for floor levels between 5 and 9, a single dummy variable for floor levels of 10 or larger, and a dummy variable for missing information on floor level), total number of floors in house (six dummy variables for the number of floors between 0 (only ground floor) and 7, one single dummy variable for 8 or more floors, and a dummy variable for missing information on the number of floors), balcony (dummy variables for yes and no and missing information), garden (dummy variables for yes and no and missing information), interaction between balcony and garden, number of bathrooms (dummy variables for 1, 2, 3, and 4 or more, and a dummy variable for missing information), guest bathroom (dummy variables for yes and no and missing information), elevator (dummy variables for yes and no and missing information), cellar (dummy variables for yes and no and missing information), built-in kitchen (dummy variables for yes and no and missing information), parking space (dummy variables for yes and no and missing information). The results from this regression are reported in Table A1 in Appendix B. The red vertical line indicates the point in time when the law "principle who orders pays" became effective (June 2015), the two gray lines indicate the points in time the rent control law became effective in Frankfurt (December 2015) and Stuttgart (November 2015). N = 219,247, data from Immobilienscout24, authors' calculations.

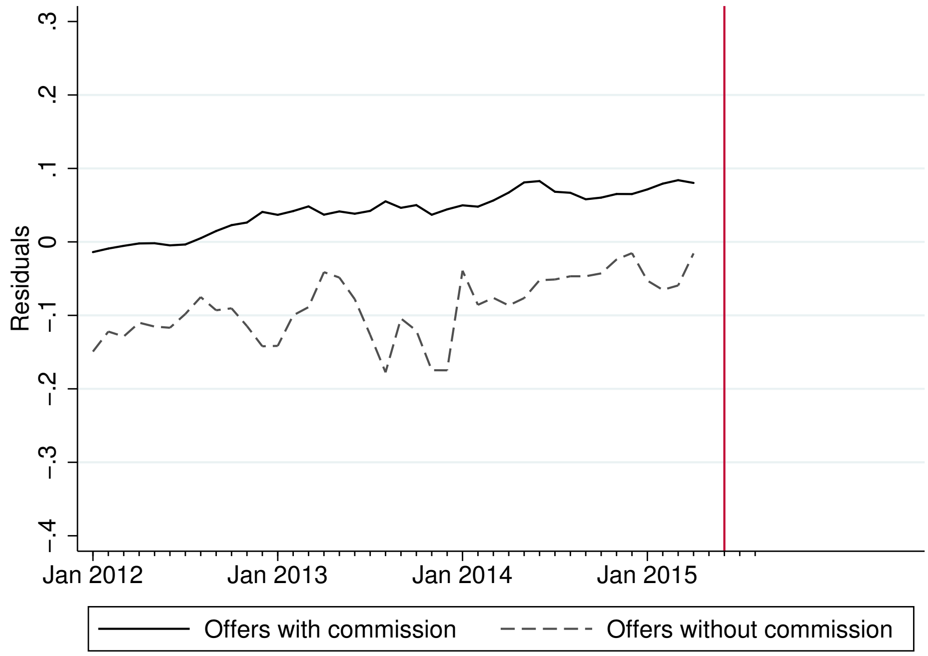
Figure 4: Residuals from a regression of ln(rental price) over time

Our main approach to estimate the effect of the reform is a difference-in-differences strategy comparing on the one hand apartments that had a commission payable by renters prior to the reform (treatment group) to apartments on the other hand that had no commission payable by renters prior to the reform (control group). Thus, our identifying assumption is parallel time trends with no shock affecting treatment and control groups differentially. To investigate the plausibility of the assumption, we graphically plot the residual of a regression of ln(rental prices) on apartment characteristics (similar as in Fig. 4) prior to the reform separately for commission apartments and no-commission apartments (Fig. 5). The rental prices of offers without a commission are lower and more volatile than the rental prices of offers including a commission for renters. The different level of prices (residuals) reflect differences in unobserved characteristics. The higher volatility of offers without commission could be due to the fact that offers without a commission are mostly posted by private persons who are less experienced than commercial offerors (REAs or firms). Only a small share among the offers without a commission prior to the reform is posted by a commercial offeror, i.e., a firm or an REA for which the landlord pays the commission. Offers with a commission are always posted by commercial offerors. Overall the price trends of offers with and without commission seem to be parallel. This provides evidence that the parallel trend assumption required for the diff-in-diffs identification is plausible for the present case.

If the standard reasoning prediction was fully true, we would expect landlords who appointed REAs prior to the reform to continue appointing REAs even after the reform, pay commissions themselves, and raise rental prices accordingly. That is, the quantity of REA service consumed would be unaffected by the reform. If, on the other hand, the behavioral prediction was true, we would expect the quantity of REA service to decrease because landlords with $(1-\theta)C < E < C$ appoint an REA prior to the reform but not after (see Section 3.2). Figure 6 plots the share of offers posted by REAs compared to private persons. We see that the share of agent offers has decreased by about 20 percentage points around the time of the reform, while that of private offers has increased by about 20 percentage points.[32] Hence, the data is rather consistent with the behavioral hypothesis. The way how the commission is paid—i.e., directly by the landlord or indirectly via a lower rent—should not affect the quantity of REA service consumed by landlords according to the standard reasoning hypothesis.

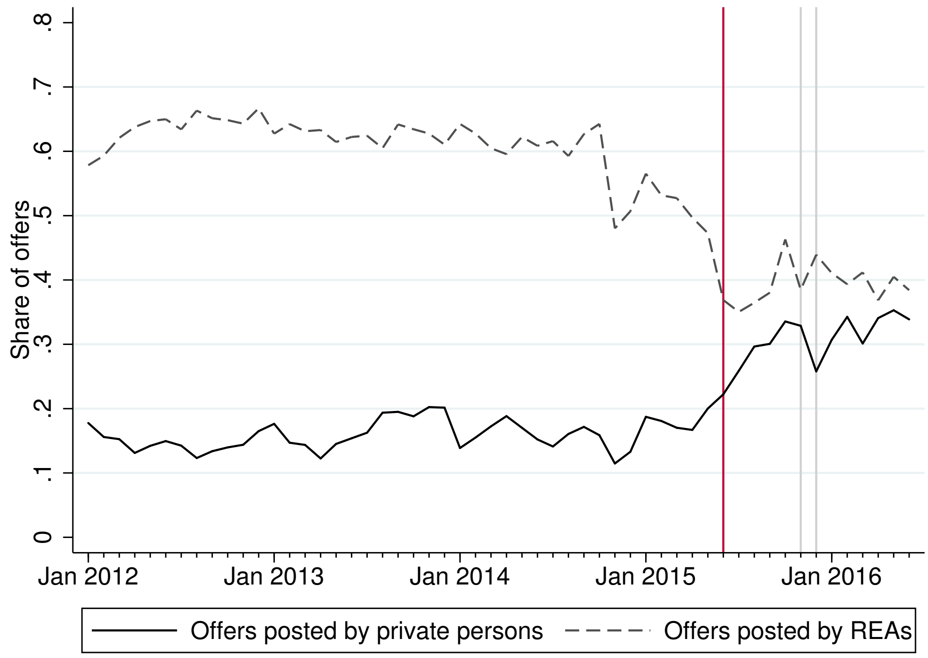
However, the declined quantity of REA service alone does not prove the behavioral hypothesis because there is another reason why the number of REAs appointed could decline even if translation between cost components was perfect. This is the case if the benefit brought by an REA (with the commission payable by the renter) for a landlord not only is the explicit service but also the “selection service” . The “selection service” consists of the fact that renters with

[32]The reason that the shares of the two lines in the figure do not sum up to one is that offers can also be posted by firms such as house constructing companies, banks, or insurances; these are neither assigned to the group of REAs nor to that of private persons.




Note: Residuals from a regression of ln(rental price) on city (dummy for Frankfurt vs Stuttgart), ln(square meters), number of rooms (dummy variables), floor level (six dummy variables for the floor levels between -1 and 4, one single dummy variable for floor levels between 5 and 9, a single dummy variable for floor levels of 10 or larger, and a dummy variable for missing information on floor level), total number of floors in house (six dummy variables for the number of floors between 0 (only ground floor) and 7, one single dummy variable for 8 or more floors, and a dummy variable for missing information on the number of floors), balcony (dummy variables for yes and no and missing information), garden (dummy variables for yes and no and missing information), interaction between balcony and garden, number of bathrooms (dummy variables for 1, 2, 3, and 4 or more, and a dummy variable for missing information), guest bathroom (dummy variables for yes and no and missing information), elevator (dummy variables for yes and no and missing information), cellar (dummy variables for yes and no and missing information), built-in kitchen (dummy variables for yes and no and missing information), parking space (dummy variables for yes and no and missing information). The red vertical line indicates the point in time when the law "principle who orders pays" became effective (June 2015). N = 174,347, data from Immobilienscout24, authors' calculations.

Figure 5: Residuals from a regression of ln(rental price) by commission status




Note: REA means real estate agent. The red vertical line indicates the point in time when the law "principle who orders pays" became effective (June 2015), the two gray lines indicate the points in time the rent control law became effective in Frankfurt (December 2015) and Stuttgart (November 2015). N = 219,247, data from Immobilienscout24, authors' calculations.

Figure 6: Share of offers posted by private persons and by REAs

a long intended rent duration are more likely to select into apartments with commission than renters with a short intended rent duration. This is due to the fact that renters with a short intended rent duration have a lower willingness to pay for a commission than renters with a long intended rent duration because the average monthly financial burden of living (monthly rent + commission divided by the number of months) is higher, the shorter the rent duration. The majority of landlords tend to prefer renters with long rent durations as every change in renters is typically associated with substantial costs for the landlord (e.g., times costs, service costs, maintenance costs). Landlords therefore might have had some positive willingness to pay for this "selection service" brought by REAs whose commission is payable by the renter. Since after the reform there is no way to make renters pay the commission, the "selection service" brought by REAs has disappeared. This could be a reason why landlords' willingness to pay for REAs has decreased and thus the quantity of REA service has declined.[33]

[33]Theoretically, landlords could design front-loaded rent contracts, which are more attractive for long-duration renters than short-duration renters, in order to achieve the same sorting affect as the REA commission payable by renters. However, interestingly, this kind of rent contracts is absolutely uncommon in Germany's housing market; rent contracts display either constant (in the majority of cases) or increasing rents (in some cases, in particular in areas with a tight rental housing market).

The question of whether landlords previously benefited from REAs separating renters according to their intended rent durations, however, is unrelated to our hypothesis testing of the rental price change. The standard reasoning hypothesis (null hypothesis) states that landlords who previously appointed an REA and made renters pay the commission should after the reform—as they can no longer make renters pay the commission—increase monthly rental prices accordingly. There is no reason why they should not increase monthly rents, knowing that renters did pay the higher price even before (in the form of the commission). The behavioral hypothesis (alternative hypothesis), in contrast, states that the reform should not (or only slightly) increase monthly rental prices of concerned apartments because of imperfect translation between commission and rent.

# 5 Method

## 5.1 Empirical Strategy

To identify the causal effect of the legal reform "principle who orders pays" on rental prices, we apply a difference-in-differences approach. According to the standard reasoning argument, the reform affects only apartments for which renters had to pay a commission prior to the reform (treatment group). Apartments for which renters never paid a commission should not be affected and hence serve as control group. We construct a panel dataset identifying apartments based on the exact address (postal code, street, and house number), the floor level,[34] the number of rooms and the size (in square meters). Each apartment identified based on these characteristics is assigned an individual apartment ID.[35] In Figure 5 it is shown that the rental price level is different for treated and untreated apartments. Hence, there seem to be unobserved differences between apartments offered with commission and apartments offered without commission. These unobserved differences could for example be related to the quality of the location or the type and condition of the building. Due to the great advantage of the panel structure of our data we are able to control for all time invariant unobserved characteristics in our estimations by including apartment fixed effects.

For 60% of the apartments (remember, one apartment has several offers, i.e., several observations in the data), all offers prior to the reform include a commission payable by the renter. For 30% of the apartments, all offers prior to the reform are without commission payable by the renter. Only for 10% of the apartments, we find some variation, i.e., some offers prior to the

[34] We allow missings in the variable floor level because for apartments in one-floor buildings this variable is often kept blank.

[35] We are aware that this might not create a perfect panel as it could be possible that two or more apartments of the same size are located in the same floor of the same building. We assume that these apartments are very similar or, at least, that differences between these apartments are not correlated with the status of being offered with or without a commission payable by the renter. Further below (Section 6) we conduct some robustness tests to check this assumption and find that it is not critical for our results.

reform include a commission payable by the renter and some offers do not. The occurrence of such "mixed" apartments can have two reasons: First, an apartment was offered once with and once without a commission payable by the renter. Second, we cannot rule out that we observe offers from two or more distinct apartments with the same apartment ID (wrong matching due to similar apartments being in the same building and same floor) and one of these apartments was offered with commission, while the other with the same apartment ID was offered without commission. Since even in the case of wrong matching the apartments within a single ID are likely to be very similar, we decided to keep them in the sample. A robustness test dropping these cases shows that our results are robust in this respect (see Section 6).

In order to keep the "mixed" apartments in the sample, we apply the following strategy: We classify apartments containing both, observations with commission and observations without commission, as partly treated. For example, assume that within one apartment ID we have two offers prior to the reform in the dataset, but these two offers relate to two different apartments (with the same size on the same floor etc.). Assume that one of these apartments is offered with and one without a commission and we observe both again after the reform. Because only one out of two observations is affected by the reform, we expect the average rental price change within this apartment ID to amount to only 50% of a comparable apartment which was fully treated. Hence, we estimate a difference-in-differences equation with the propensity to have a commission prior to the reform, i.e.:

$$ln(P_{it}) = \beta_i + \gamma R_t + \delta T_i R_t + \lambda t + \epsilon_{it},$$

where $P_{it}$ is the monthly rental price of apartment $i$ offered at time $t$, $\beta_i$ are the apartment fixed effects, $R_t$ is a dummy variable taking on the value 1 if the offer is published after the reform (June 2015) and 0 otherwise, $t$ captures the time trend, $T_i$ is the propensity to be treated, i.e., the variable takes on values between 0 and 1; it gives the number of offers of apartment $i$ prior to the reform for which a commission was payable by the renter divided by the number of all offers of apartment $i$ prior to the reform. Hence, $\delta$ is the coefficient of main interest: the behavioral hypothesis suggests that $\delta < 0.0198$ (i.e., in terms of the sufficient statistic of section 3, $\theta > 0$), in the extreme case, that $\delta = 0$ (i.e., $\theta = 1$). The standard reasoning prediction, in contrast, suggests that $\delta = 0.0198$ (null hypothesis), i.e., in terms of the sufficient statistic, $\theta = 0$). Rejecting the null hypothesis and finding that $\delta < 0.0198$ supports the behavioral hypothesis exposed in Section 1.

Applying the outlined diff-in-diffs strategy ensures to disentangle between the reform effect and an (unknown) time effect at the exact date of the reform. Without a control group, one could estimate the reform effect merely as the rental price change at the time of the reform in June 2015 for the average apartment. First, this would attenuate the results since there is no reform-induced rent increase predicted for untreated apartments. Second, accounting for the time trend in a linear way—or even including different polynomials—would never perfectly capture the time effect, which can have a ragged shape as is suggested by Figures 3 and 4. This could lead to serious distortion. In contrast, estimating $\delta$ in our diff-in-diffs framework will account for any overall rental price change at the time of June 2015 that is not related to the reform. Our identifying assumption is that, at the time of the reform, there is no other shock affecting rental prices of treated but not of control apartments.

## 5.2 The Sample with Apartment Fixed Effects

Our original sample consists of 219,247 apartment offer observations. Restricting the sample to observations for which the apartment could be identified[36] reduces the sample size to 142,241 observations from 83,918 apartments (groups in the panel dataset). Among these, for 23,274 apartments (containing 78,846 observations) the sample contains at least two offers and at least one before the reform (this is necessary to determine the treatment status of the apartment). Since we have data only for a relatively short time period (years 2012 to 2016), this makes our sample even more restrictive with respect to mean rent durations: rent durations are shorter in our sample than in the overall population. Thus, we should detect an even higher price increase due to the reform if the standard reasoning prediction was true. Only those apartments which are observed at least once before *and* once after the reform (17,604 offers from 4,733 apartments) contribute to the estimation of our diff-in-diffs coefficient $\delta$. However, we do not restrict the sample to the 17,604 observations because all the 78,846 observations contribute to estimating the other coefficients and thus improve precision of the estimates.

Table A2 in Appendix B reports summary statistics for the panel sample used for the diff-in-diffs estimation. Because apartments without observations prior to the reform are excluded, the percentage of offers after the reform has decreased to 9.8% (compared to 18.7% in the full sample as shown in Table 1). The same reason explains why the share of observations with commission has increased to 67.7% (compared to 54.7% in the full sample), that of private offers has decreased to 10% (compared to 18.6% in the full sample), and the share of offers posted after the introduction of the rent control law has decreased to 3.8% (compared to 9.2% in the full sample). All other apartment characteristics are fairly similar between the panel sample used for the diff-in-diffs estimation (presented in Table A2 in Appendix B) and the full sample (presented in Table 1).

[36] As explained in Section 5.1, identification of apartments is effectuated based on postal code, name of street, house number, floor level, size in square meters, and number of rooms. Furthermore, to avoid bias due to reposts of offers, we exclude apartments above the 95-percentile with respect to the number of posts prior to the reform. The results are qualitatively the same even without excluding these apartments from the data.

# 6 Results

The main results based on the regression equation described in Section 5.1 are reported in Table 2. The estimations include apartment fixed effects and a dummy variable indicating whether the offer is affected by the rent control law described in Section 4.1. The time trend is specified linearly; this restriction is released further below. The coefficient of main interest is the interaction between the post reform dummy and the probability to be treated, i.e., the difference-in-differences estimator $\delta$, reported in the first row of the table.

Column (1) contains the results of our main specification, including the entire sample. Based on a Wald test we reject the null hypothesis $H_0$: $\delta \geq 0.0198$ (standard reasoning hypothesis) at any conventional significance level (see the last row of the table). This means that we reject the hypothesis of the reform making rents of treated apartments increase by 1.98%. This speaks in favor of the behavioral hypothesis stating that the rental price increase is smaller or even zero. Given that the $\delta$ coefficient is not significantly different from zero and has even a negative sign, we cannot reject the extreme behavioral hypothesis of the reform not having increased rental prices at all.

Table 2: The effect of the reform on rental prices

| | (1) Joint sample | (2) Frankfurt | (3) Stuttgart |
|---|---|---|---|
| Post reform × commission | -0.0033 | -0.0005 | -0.0092 |
| | (0.004) | (0.005) | (0.010) |
| Post reform | -0.0017 | -0.0068 | 0.0139 |
| | (0.004) | (0.004) | (0.012) |
| Time trend $t$ | 0.0017$^{***}$ | 0.0017$^{***}$ | 0.0018$^{***}$ |
| | (0.000) | (0.000) | (0.000) |
| Observations | 78846 | 58121 | 20725 |
| Groups | 23274 | 16484 | 6790 |
| P-value from testing $H_0$: $\delta \geq 0.0198$ | <0.001 | <0.001 | 0.003 |

Note: The dependent variable in all estimations is ln(monthly rental price). All estimations include apartment fixed-effects and a dummy variable for offers subject to the rent control law. *Post reform* is a dummy variable taking on the value 1 if the offer is published in June 2015 or later. *Post reform × commission* is an interaction between the variable *Post reform* and the probability (value between 0 and 1) that an offer for this apartment had a commission prior to the reform (probability to be treated). Standard errors in parentheses are clustered on the apartment level. $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, $^{***}$ $p < 0.001$. Data from Immobilienscout24 2012–2016, authors' calculations.

Translated to the $\theta$ parameter of the sufficient statistic introduced in section 3, we reject the null hypothesis $H_0 : \theta = 0$ (for $D = 120$) and we cannot reject the (extreme behavioral) hypothesis $H_1 : \theta = 1$. Since we find an effect on rents of -0.0033%, the point estimate for

$\theta$ is 1.17.[37] The 95% lower confidence bound for $\theta$ results to be 0.73. Thus, we conclude that there is an essential share of non-translation between cost components in the rental pricing mechanism (remember, $\theta = 1$ would mean no translation, i.e., full inattention or fully separated mental accounts).

Obviously, the exact price increase predicted by the standard reasoning hypothesis strongly depends on the mean expected rent duration assumed. Because there is no suitable statistic in Germany on the expected rent duration nor even on the *completed* rent duration, we had to use a plausible estimate that is rather conservative regarding our hypothesis test in order to make sure not to overreject. When choosing a value for $D$, one has to be aware of the fact that the mean *expected* rent duration for a specific apartment offered on the market is systematically shorter than the mean *completed* rent duration because short-duration renters move into new apartments more frequently than long-duration renters; hence, a landlord offering her apartment is more likely to encounter a short-duration renter even in a situation where the numbers of short- and long-duration renters are equal. The only statistic loosely related to what we need can be found in a specialized survey about housing of the German Microcensus 2010.[38] According to that, in the German western federal states, 19% of households have lived in their currently rented apartment for less than two years, more than 57% have lived in their currently rented apartment for less than eight years, and more than 79% have lived in their currently rented apartment for less than 20 years (cumulative probabilities).[39] These numbers suggest that there is considerable movement on the rental housing market and our arbitrary assumption of the mean expected rent duration being ten years seems to be plausible or rather conservative.

Nevertheless, we check the sensitivity of our results with respect to the assumed mean expected rent duration.

First, we calculate the $\theta$ parameter for different rent durations. As mentioned, assuming $D = 120$ (10 years) makes us find the 95% lower confidence bound for $\theta$ to be 0.73. Assuming, instead $D = 60$ (5 years), the lower bound for $\theta$ is 0.87; assuming D=180 (15 years), the lower bound is 0.60. This illustrates that, even if we assume a fairly long mean expected rent duration, we find a substantial degree of non-translation between cost components.

Second, we calculate the maximal mean expected rent duration for which we would still reject the null hypothesis $H_0$: $\delta \geq 0.0198$ at the 5% significance level. This turns out to be 595 months, i.e., around 50 years. This means that only if we assumed a mean expected rent duration of more than 50 years, we were no longer be able reject the null hypothesis. A duration of 50 years, however, is far longer than what the mean expected rent duration can plausibly be.

[37] This is the result from solving $-0.0033P = (1-\theta)\frac{C}{D}$, plugging in $C = 2.38P$ and $D = 120$.

[38] Federal Statistical Office (Statistisches Bundesamt) 2012, Bauen und Wohnen, Mikrozensus-Zusatzerhebung 2010, Bestand und Struktur der Wohneinheiten, Wohnsituation der Haushalte, Fachserie 5 Heft 1.

[39] The period of time living in the currently rented apartment has been surveyed by the German Federal Statistical Office only by brackets as presented here.

This result illustrates that we must reject the standard reasoning hypothesis even when assuming very long mean expected rent durations.

In column (2) and (3) of Table 2 we report results from separate estimations for the two cities. The $\delta$ coefficient is not significantly different from zero for either city. The point estimate is smaller (more negative) for the Stuttgart sample, but due to the substantially smaller sample size, the p-value of the Wald test for the null hypothesis is slightly higher. Finally, for both cities we can again reject $H_0 : \delta \geq 0.0198$ at any conventional significance level.

Locations and apartments are very heterogeneous. We were concerned about the possibility that outliers or certain groups of apartments due to irregular pricing trends prevent us from identifying the reform-induced rent increase. In a further step, we therefore focus our investigation on apartments for which standard reasoning predicts the reform-induced rent increase to be strongest.

First, these are small apartments because small apartments on average have a higher renter turnover than large apartments; this means, that mean expected rent duration ($D$) should be smaller and thus the reform effect should be stronger. In the estimation reported in column (1) of Table 3 the sample is restricted to apartments smaller or equal to 55 square meters (smallest 25% of the sample). The results are very similar to our main results; not even for these apartments we find reform-induced rental price increases.

Second, another group of apartments that usually have a high renter turnover are apartments in inner-city districts. Furthermore, the dynamics in the rental markets in inner-city districts are often argued to be particularly strong. Thus, increasing rents should be particularly easily feasible by landlords offering apartments in these areas. Results from estimations based on the sample restricted to apartments located in inner-city districts[40] are reported in column (2) of Table 3. Even here, we do not find a significant rent increase and we have to reject the standard reasoning hypothesis in favor of the behavioral hypothesis.

We perform a series of further analyses to test the robustness of our results with respect to a number of concerns.

The first concern is that professional REAs have better knowledge about market rents compared to (private) landlords and this could be a reason why apartments with a commission have higher rental prices (as offers by REAs come closer to the maximally possible rent). We call this the consultancy effect. The consultancy effect could be a reason for the rental price gap between commission- and no-commission-apartments shown in Figure 5. Landlords who stop appointing an REA at the time of the reform might lack expert knowledge about market prices and thus eventually ask prices below the apartments' potential. This could be a reason for the reform-induced price increase to be smaller than otherwise expected. In column (3) of Table 2 we address this concern by reducing the sample in the following way: Within the treatment group we include only those apartments that have post-reform offers posted by REAs. We thus

[40]Where inner-city districts are defined by the city administrations.

exclude within the treatment group all apartments that are offered by private persons after the reform. This keeps the consultancy effect constant between before and after the reform. As can be seen in Figure 6 above, not all landlords quit appointing REAs at the time of the reform. Those who continue appointing an REA after the reform have to pay the commission themselves. Our results based on the described restricted sample turns out to be robust: Even those landlords who decided to continue appointing REAs after the reform do not increase rental prices. This means that we reject the standard reasoning hypothesis, while not being able to reject the extreme behavioral hypothesis of no rental price change compared to the control group.

A second concern could be that the reform effect did not step in immediately but took some time to adjust. In alternative estimations in Table 4, we therefore drop all observations of offers published in the first month (column (4)) or in the first two months (column (5)) after the date the reform became effective. The results appear to be very similar to our baseline results. In order to further investigate whether the effect on rental prices appears on the even longer run, we drop all observations from June 2015 until the end of 2015 (column (6)). The results appear to be robust even when allowing for very slow adjustment: even after half a year we cannot detect any rent increase that can be attributed to the reform.

A third concern relates to the possibility of anticipation effects based on the announcement of the law reform "principle who orders pays". The law was passed in the parliament in April 2015. Theoretically, there is no economic reason why the pure announcement of the reform should immediately affect rental prices. Nevertheless, we are concerned that landlords (irrationally) reacted already shortly before the reform became actually effective. To test our results for robustness against anticipation effects, we drop from the sample the first month (column (7) of Table 3) and the first two months (column (8)) prior to the reform. The estimation results do not change compared to our main specification.

A fourth concern is that the results were driven by the so-called "mixed" apartments (see Section 5.1). Apartments that are sometimes marketed by REAs and sometimes without could be different in terms of apartment characteristics, landlords, or REAs, compared to the majority of apartments being marketed either always or never by REAs. We therefore restrict our sample to only those apartments where offers prior to the reform were either always or never with a commission, i.e., apartments which are either fully treated ($T$=1) or not treated ($T$=0). The results are very similar to our main results, i.e., they do not seem to be driven by "mixed" apartments (see Table 2, column (9)).

Table 3: The effect of the reform on rental prices—sample restrictions

| | (1) Size ≤55 sqm | (2) Only inner-city | (3) REA before and after reform | (4) Drop June 2015 | (5) Drop June+July 2015 | (6) Drop June-Dec. 2015 | (7) Drop May 2015 | (8) Drop April+May 2015 | (9) Always or never commission | (10) Drop rental control |
|---|---|---|---|---|---|---|---|---|---|---|
| Post reform × commission | -0.0115 | 0.0012 | -0.0039 | -0.0041 | -0.0023 | -0.0036 | -0.0050 | -0.0053 | -0.0021 | -0.0027 |
| | (0.006) | (0.007) | (0.005) | (0.005) | (0.005) | (0.007) | (0.004) | (0.005) | (0.004) | (0.006) |
| Post reform | 0.0141** | -0.0093 | 0.0007 | 0.0004 | 0.0004 | 0.0037 | 0.0009 | 0.0043 | -0.0012 | -0.0020 |
| | (0.005) | (0.007) | (0.004) | (0.005) | (0.005) | (0.012) | (0.004) | (0.004) | (0.004) | (0.005) |
| Time trend t | 0.0023*** | 0.0017*** | 0.0017*** | 0.0017*** | 0.0017*** | 0.0015*** | 0.0017*** | 0.0017*** | 0.0018*** | 0.0017*** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| Observations | 21596 | 41071 | 74808 | 77547 | 76556 | 73849 | 77227 | 75590 | 70602 | 75814 |
| Groups | 6983 | 11565 | 22196 | 23274 | 23274 | 23274 | 23212 | 23036 | 21105 | 23274 |
| P-value from testing $H_0$: $\delta \geq 0.0198$ | <0.001 | 0.006 | <0.001 | <0.001 | <0.001 | <0.001 | <0.001 | <0.001 | <0.001 | <0.001 |

Note: The dependent variable in all estimations is ln(monthly rental price). All estimations include apartment fixed-effects and a dummy variable for offers subject to the rent control law. *Post reform* is a dummy variable taking on the value 1 if the offer is published in June 2015 or later. *Post reform × commission* is an interaction between the variable *Post reform* and the probability (value between 0 and 1) that an offer for this apartment had a commission prior to the reform (probability to be treated). Standard errors in parentheses are clustered on the apartment level. $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, $^{***}$ $p < 0.001$. Data from Immobilienscout24 2012–2016, authors' calculations.

A fifth concern relates to the potential bias generated by the rent control law implemented at the end of the year 2015. To account for the effect of this law on rental prices, a control variable is included in all estimations presented. An alternative way of accounting for this law in the analysis is to drop all apartment offers published in the period concerned by the law. This is done in the estimation reported in column (10) of Table 3. Here, we drop observations from October 2015 or later.[41] The results are barely different from the main specification.

A sixth concern relates to the specification of the time trend. Note that in all presented specifications we find a strongly significant positive time trend in rental prices; hence, the time trend plays an important role. In order to test the robustness of our results with respect to the specification of the time trend, we stepwise include the second and the third polynomial of $t$ in the regression (Table 4, columns (2) and (3); for convenience, column (1) repeats the main specification that was already reported in column (1) of Table 2). We find the regression coefficients related to the polynomials of order 2 and 3 to be statistically insignificant (also jointly) and thus conclude that the linear specification captures the main trend. More importantly, the estimate of $\delta$ is robust: it is not significantly different from zero in either specification and the null hypothesis $H_0$: $\delta \geq 0.0198$ is again rejected at any conventional significance level. Allowing for seasonal effects (by adding dummy variables for each month of the year) instead of the polynomials (column (4)) or in addition to the polynomials (column (5)) does not change the picture. Including dummy variables for each month in the sample (column (6)) or allowing the linear time trend to differ between the cities Frankfurt and Stuttgart (column (7)) does not either.

To sum up, we find no evidence of a reform-induced rental price increase as predicted by standard economic reasoning. In contrast, we cannot reject the extreme behavioral hypothesis of a zero rent increase. The lack of landlords passing the burden of the commission back to renters via increased monthly rents is called imperfect translation between cost components. This imperfect translation implies that landlords appointing an REA prior to the reform (and making renters pay the commission) did not bear the (full) cost of the REA service, even though they alone benefited from the service. This implies that, prior to the reform, the quantity of REA service consumed by landlords was higher than optimal. Thus, the reform not only unburdened renters but improved market efficiency by reducing the inefficiently high level of REA service consumed.

[41] We chose October instead of November because offers published in one month are typically for contracts to be concluded in the following month.

Table 4: The effect of the reform on rental prices—different specification of the time trend and seasonal effects

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| Post reform x commission | -0.0033 | -0.0030 | -0.0030 | -0.0034 | -0.0031 | -0.0031 | -0.0031 |
| | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) |
| Post reform | -0.0017 | 0.0003 | -0.0010 | -0.0018 | 0.0000 | | -0.0010 |
| | (0.004) | (0.005) | (0.005) | (0.004) | (0.005) | | (0.004) |
| Time trend t | 0.0017*** | 0.0020*** | 0.0025*** | 0.0017*** | 0.0024*** | | 0.0907 |
| | (0.000) | (0.000) | (0.001) | (0.000) | (0.001) | | (0.094) |
| $t^2/1.000$ | | -0.0067 | -0.0327 | | -0.0265 | | |
| | | (0.006) | (0.023) | | (0.024) | | |
| $t^3/10.000$ | | | 0.0037 | | 0.0026 | | |
| | | | (0.003) | | (0.003) | | |
| Frankfurt x t | | | | | | | -0.0000 |
| | | | | | | | (0.000) |
| Seasonal effects | | | | X | X | | |
| Monthly dummies | | | | | | X | |
| Observations | 78846 | 78846 | 78846 | 78846 | 78846 | 78846 | 78846 |
| Groups | 23274 | 23274 | 23274 | 23274 | 23274 | 23274 | 23274 |
| P-value from testing $H_0$: $\delta \geq 0.0198$) | <0.001 | <0.001 | <0.001 | <0.001 | <0.001 | <0.001 | <0.001 |

Note: The dependent variable in all estimations is ln(monthly rental price). All estimations include apartment fixed-effects and a dummy variable for offers subject to the rent control law. *Post reform* is a dummy variable taking on the value 1 if the offer is published in June 2015 or later. *Post reform* × *commission* is an interaction between the variable *Post reform* and the probability (value between 0 and 1) that an offer for this apartment had a commission prior to the reform (probability to be treated). *Seasonal effects* include dummy variables for each month of the year (i.e., Jan to Dec, excluding a reference month). *Monthly dummies* include dummy variables for each month included in the data set (i.e., Jan2012 to Dec2016, excluding a reference month). Standard errors in parentheses are clustered on the apartment level. $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, $^{***}$ $p < 0.001$. Data from Immobilienscout24 2012–2016, authors' calculations.

Certainly, this conclusion only holds under the assumption that REAs do not improve market efficiency. This could happen, in particular, if REAs shortened the time of rental offers on the market and thus lowered the vacancy rate. However, in many cities vacancy rates of rental housing are minuscule and thus there is little room for REAs to accelerate the marketing process. In the two cities considered in this paper, vacancy rates were around 1% or below both before and after the reform in 2015. According to data from the Federal Institute for Research on Building, Urban Affairs and Spatial Development (Bundesinstitut für Bau-, Stadt- und Raumforschung—BBSR) the vacancy rate of residential housing in Frankfurt was between 0 and 1% in the years 2014 to 2016; in Stuttgart, the rate was between 1 and 2% in 2014 and between 0 and 1% in the years 2015 and 2016. This is consistent with data from Empirica Regionaldatenbank (*CBRE-empirica-Leerstandsindex*) reporting vacancy rates for Frankfurt of 0.6% and 0.5% in the years 2014 and 2016, respectively; in Stuttgart the numbers are 1.0% and 0.7% for the years 2014 and 2016, respectively. These numbers suggest that the decrease in REA services consumed did not increase vacancy rates. Hence, the law under consideration is likely to have improved market efficiency.

# 7 Conclusion

In this paper we investigate the effect of the legal reform "principle who orders pays" on rental prices. The reform has been implemented with the goal of unburdening renters by prohibiting landlords to make renters pay for REAs appointed by landlords. Standard economic reasoning claims the economic incidence of a burden being independent of the payment liability; this would imply that rental prices of concerned apartments increase as a result of the legal reform and that the reform did not unburden renters. Evidence from behavioral economics, in contrast, provides arguments against the liability side equivalence.

We present a model leaned on Chetty et al. (2009) and DellaVigna (2009) who model inattention to some opaque component of a variable compared to full attention to the visible component of that variable. This allows us to find the sufficient statistic $\theta$ that, in our context, does not need to be interpreted narrowly as inattention parameter but more generally measures the extent of imperfect translation between cost components affecting demand. This imperfect translation could, for instance, be the result of mental accounting, cognitive limitations, or inattention. We estimate this sufficient statistic based on reduced-form regressions.

Based on our results from difference-in-differences analyses of data from an online real estate marketplace we reject the standard reasoning hypothesis in favor of the behavioral hypothesis: We find no evidence of a reform-induced rental price increase, not even for particularly susceptible groups of apartments and not even in the long-run. This means that we find evidence for a substantial lack of translation of the size of at least 73% (95% confidence bound). Our finding is robust to a number of sensitivity checks with respect to consultancy effects, adjustment effects, anticipation effects, and the specification of the time trend.

Our findings have important implications for policy as we can conclude that policy reforms as the one under consideration can actually affect the burden of the market sides. In particular in the context of the rental housing market this is a significant finding for several reasons: First, expenditure for rental housing makes up an important share of the available income, in particular for poor households. Second, renters on average are much less wealthy than owners; thus, unburdening renters plays an important role in distribution policies. Third, other policy measures to unburden renters, in particular rent control policies, have shown distorting and undesirable effects on the market (Kholodilin and Licheron, 2017; Kholodilin et al., 2016; Mense et al., 2017; Sims, 2007; Thomschke, 2016).

Furthermore, since we find the translation between REA commission and monthly rent to be imperfect and landlords prior to the reform used to pass the payment liability of REA commissions to renters, we conclude that the quantity of REA service consumed prior to the reform was higher than the efficient level. The reform achieved to reduce this inefficiently high demand by making landlord internalize the full costs of the service they consume. The reform thus has improved market efficiency and increased overall social welfare.

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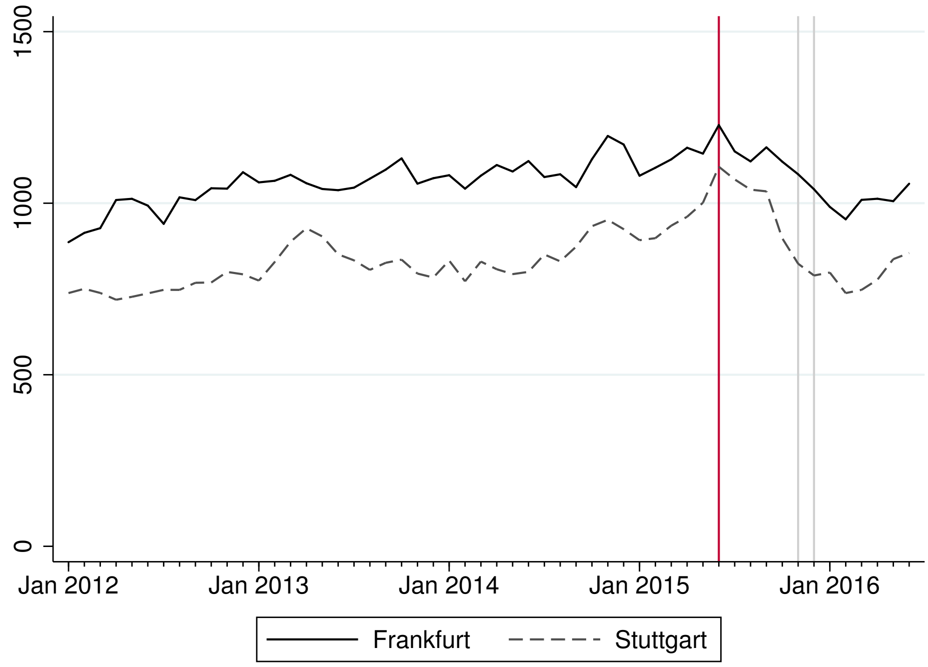
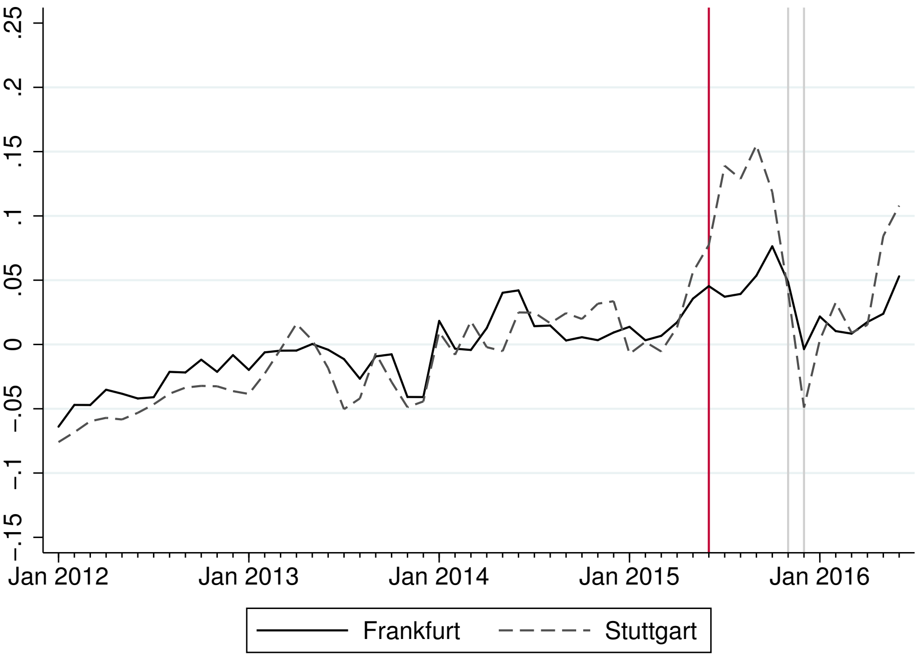
# Appendix A: Descriptive Analysis Over Time By City



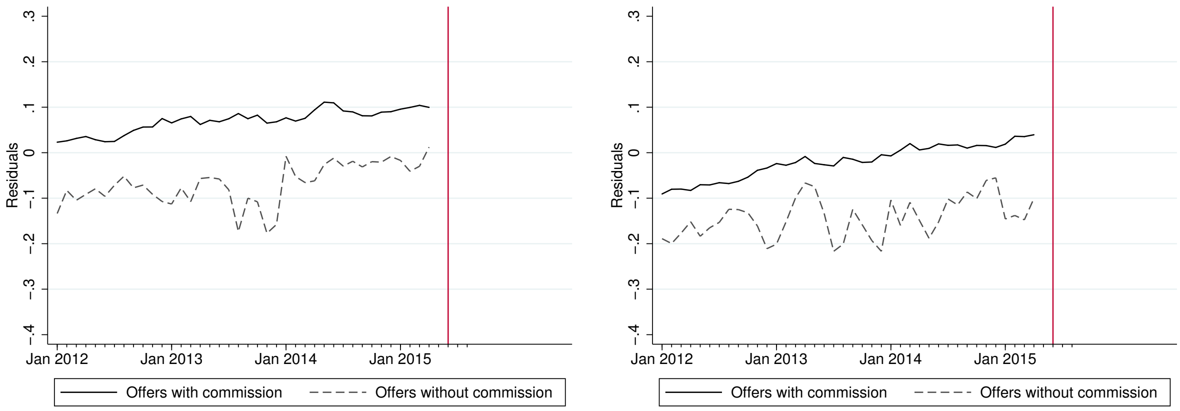

Note: The red vertical line indicates the point in time when the law “principle who orders pays” became effective (June 2015), the two gray lines indicate the points in time the rent control law became effective in Frankfurt (December 2015) and Stuttgart (November 2015). N = 219,247, data from Immobilienscout24, authors’ calculations.

Figure A1: Mean rental prices over time by city




Note: Residuals from a regression of ln(rental price) on ln(square meters), number of rooms (dummy variables), floor level (six dummy variables for the floor levels between -1 and 4, one single dummy variable for floor levels between 5 and 9, a single dummy variable for floor levels of 10 or larger, and a dummy variable for missing information on floor level), total number of floors in house (six dummy variables for the number of floors between 0 (only ground floor) and 7, one single dummy variable for 8 or more floors, and a dummy variable for missing information on the number of floors), balcony (dummy variables for yes and no and missing information), garden (dummy variables for yes and no and missing information), interaction between balcony and garden, number of bathrooms (dummy variables for 1, 2, 3, and 4 or more, and a dummy variable for missing information), guest bathroom (dummy variables for yes and no and missing information), elevator (dummy variables for yes and no and missing information), cellar (dummy variables for yes and no and missing information), built-in kitchen (dummy variables for yes and no and missing information), parking space (dummy variables for yes and no and missing information). The red vertical line indicates the point in time when the law "principle who orders pays" became effective (June 2015), the two gray lines indicate the points in time the rent control law became effective in Frankfurt (December 2015) and Stuttgart (November 2015). N = 219,247, data from Immobilienscout24, authors' calculations.

Figure A2: Residuals from a regression of ln(rental price) over time by city

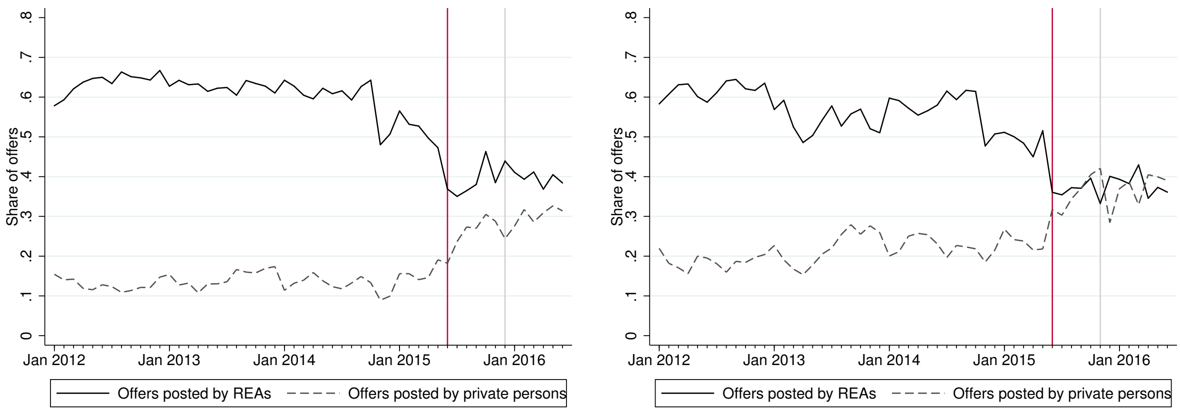



(a) Frankfurt

(b) Stuttgart

Note: Residuals from a regression of ln(rental price) on ln(square meters), number of rooms (dummy variables), floor level (six dummy variables for the floor levels between -1 and 4, one single dummy variable for floor levels between 5 and 9, a single dummy variable for floor levels of 10 or larger, and a dummy variable for missing information on floor level), total number of floors in house (six dummy variables for the number of floors between 0 (only ground floor) and 7, one single dummy variable for 8 or more floors, and a dummy variable for missing information on the number of floors), balcony (dummy variables for yes and no and missing information), garden (dummy variables for yes and no and missing information), interaction between balcony and garden, number of bathrooms (dummy variables for 1, 2, 3, and 4 or more, and a dummy variable for missing information), guest bathroom (dummy variables for yes and no and missing information), elevator (dummy variables for yes and no and missing information), cellar (dummy variables for yes and no and missing information), built-in kitchen (dummy variables for yes and no and missing information), parking space (dummy variables for yes and no and missing information). The red vertical line indicates the point in time when the law "principle who orders pays" became effective (June 2015). N = 174,347, data from Immobilienscout24, authors' calculations.

Figure A3: Residuals from a regression of ln(rental price) by commission status—by city




(a) Frankfurt

(b) Stuttgart

Note: REA means real estate agent. The red vertical line indicates the point in time when the law "principle who orders pays" became effective (June 2015), the two gray lines indicate the points in time the rent control law became effective in Frankfurt (December 2015) and Stuttgart (November 2015). N = 219,247, data from Immobilienscout24, authors' calculations.

Figure A4: Share of offers posted by REAs and private persons—by city

# Appendix B: Additional Tables

Table A1: Regression of ln(rental price) on apartment characteristics

| | | |
|---|---|---|
| Frankfurt (vs Stuttgart) | 0.103*** | (0.001) |
| ln(sqm) | 0.916*** | (0.003) |
| Balcony | 0.028*** | (0.002) |
| Balcony=*missing* | -0.002 | (0.017) |
| Garden | 0.015*** | (0.003) |
| Garden=*missing* | 0.030*** | (0.005) |
| Balcony × garden | -0.031*** | (0.004) |
| Balcony × garden=*missing* | 0.010** | (0.005) |
| Balcony=*missing* × garden | 0.065*** | (0.020) |
| Balcony=*missing* × garden=*missing* | 0.105*** | (0.017) |
| Guest bathroom | 0.045*** | (0.002) |
| Guest bathroom=*missing* | 0.026*** | (0.002) |
| Elevator | 0.079*** | (0.002) |
| Elevator=*missing* | -0.023*** | (0.003) |
| Cellar | -0.032*** | (0.001) |
| Cellar=*missing* | 0.040*** | (0.003) |
| Built-in kitchen | 0.129*** | (0.001) |
| Built-in kitchen=*missing* | 0.009*** | (0.003) |
| Parking space | -0.055*** | (0.009) |
| Constant | 2.616*** | (0.028) |
| Dummies # rooms | yes | |
| Dummies # bathrooms | yes | |
| Dummies floor level | yes | |
| Dummies # floors | yes | |
| N | 219247 | |
| R squared | 0.800 | |

The results are based on ordinary least squares estimates. Dummy variables for the number of rooms are included starting from 1 to 13 allowing for half rooms. Dummy variables for the number of bathrooms are included (1 bathroom, 2 bathrooms, 3 or more bathrooms, information on number of bathrooms missing). Dummy variables for the floor level are included starting from the souterrain to the 5th level; the floor levels 6 to 10 are integrated in a single dummy variable, the same for levels 11 and higher; missing values are accounted for by another dummy variable. The total number of floors is also controlled for by a set of dummy variables starting from 0 to 7; the number of floors being 8 or higher is integrated in a single dummy variable; missing values are accounted for by another dummy variable. Standard errors are in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Data from Immobilienscout24 2012–2016, authors' calculations.

Table A2: Summary statistics–panel sample

| Variable | Mean | Std. Dev. | Min. | Max. | N |
|---|---|---|---|---|---|
| Rental price (in euros) | 985.293 | 687.97 | 130 | 9000 | 78846 |
| Price per sqm (in euros) | 12.068 | 3.344 | 4 | 30 | 78846 |
| Post reform | 0.098 | 0.297 | 0 | 1 | 78846 |
| Commission | 0.677 | 0.468 | 0 | 1 | 78659 |
| Frankfurt (vs Stuttgart) | 0.737 | 0.44 | 0 | 1 | 78846 |
| Number of rooms | 2.644 | 1.102 | 1 | 11 | 78846 |
| Size in square meters | 80.043 | 40.231 | 10 | 400 | 78846 |
| Floor level | 2.385 | 2.032 | -1 | 24 | 69375 |
| Total number of floors in house | 4.357 | 2.49 | 0 | 50 | 54917 |
| Balcony | 0.701 | 0.458 | 0 | 1 | 70745 |
| Garden | 0.171 | 0.376 | 0 | 1 | 58342 |
| Number of bathrooms | 1.184 | 0.406 | 1 | 4 | 50982 |
| Guest bathroom | 0.29 | 0.454 | 0 | 1 | 69932 |
| Elevator | 0.391 | 0.488 | 0 | 1 | 65260 |
| Cellar | 0.711 | 0.453 | 0 | 1 | 74678 |
| Built-in kitchen | 0.748 | 0.434 | 0 | 1 | 67645 |
| Parking space | 0.001 | 0.038 | 0 | 1 | 78846 |
| Inner-city | 0.521 | 0.5 | 0 | 1 | 78846 |
| Rent control | 0.038 | 0.192 | 0 | 1 | 78846 |
| Private offer | 0.1 | 0.3 | 0 | 1 | 76843 |
| Offer by REA | 0.658 | 0.474 | 0 | 1 | 76843 |
| Offer by other commercial entity | 0.236 | 0.425 | 0 | 1 | 78846 |

Data from Immobilienscout24 2012–2016, authors' calculations.